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THE

Philosophy of Mathematics

WITH SPECIAL REFERENCE TO

THE ELEMENTS OF GEOMETRY AND THE INFINITESIMAL METHOD.

BY

ALBERT TAYLOR BLEDSOE, A.M., LL.D.,

LATE PROFESSOR OF MATHEMATICS IN THE UNIVERSITY OF VIRGINIA.

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CHAPTER I.

FIRST PRINCIPLES OF THE INFINITESIMAL METHOD—THE METHOD OF EXHAUSTION.

The student of mathematics, on passing from the lower branches of the science to the infinitesimal analysis, finds himself in a strange and almost wholly foreign department of thought. He has not risen, by easy and gradual steps, from a lower into a higher, purer, and more beautiful region of scientific truth. On the contrary, he is painfully impressed with the conviction, that the continuity of the science has been broken, and its unity destroyed, by the influx of principles which are as unintelligible as they are novel. He finds himself surrounded by enigmas and obscurities, which only serve to perplex his understanding and darken his aspirations after knowledge. That clearness of evidence, which is the boast of the mathematics, and which has hitherto cheered and stimulated his exertions, forsakes him as soon as he enters on the study of the infinitesimal calculus, and the darkness of doubt settles on his path. If, indeed, he does not

abandon the study in disgust or despair, as thousands have done, he pursues it for the sake of a diploma or a degree, or from some less worthy motive than the love of science. He certainly derives from it comparatively little advantage in the cultivation of his intellectual powers; because the dark and unintelligible processes he is required to perform scarcely demand a natural exercise of them.

These disadvantages of the study are due, for the most part, to the manner in which the calculus is usually taught. In most elementary works on the differential calculus, the first principles of the science are not set forth at all, or else so imperfectly defined as to mislead the student from the clear path of mathematical science into a region of clouds and darkness. I have frequently made the experiment with some of the best of such works. I have more than once put them into the hands of a class of from ninety to a hundred students, among whom there were mathematical minds of no ordinary power, and required them to tell me what are the first principles of the infinitesimal method or calculus. Yet, after having read and mastered the first chapter, which, of course, contained a discussion of "First Principles," not one of them had acquired the least notion of what those principles are. Not one of them could even name the first principles of the science, much less define them. In this respect, the most capable and diligent members of the class were on a perfect level with the most stupid and indolent. Indeed, if the authors of the books themselves knew what the first principles of the calculus are, they were very careful not to unveil their knowledge.

Now, the very first condition of the existence of a mathematical science *as such* is, that its first principles shall be so clear and so perfectly defined that no one could mistake them. But even this primary and indispensable condition is not fulfilled by most of the treatises or text-books on the infinitesimal analysis. Hence this analysis, as usually developed in books for the instruction of beginners, is still in a semi-chaotic state. If, then, we would introduce anything like the order, harmony, and beauty of real mathematical science into the transcendental analysis, the first step to be taken is to exhibit its first principles in a clear and unmistakable light. My object in this work is to contribute all in my power toward so desirable a result; or, in other words, to render as clear as possible the fundamental principles of the higher calculus, from which the whole science should be seen to flow in the form of logical consequence, and that, too, as clearly as the light of day flows from the sun. Much has already been done in this direction; far—far more than has been appropriated by the so-called teachers of the science. Hence I shall have frequent occasion to avail myself of the labors of others; but I shall never do so without an explicit acknowledgment of my obligation to them.

In the prosecution of this design, I shall trace the rise and progress of the infinitesimal analysis from the first appearance of its elements in the Greek geometry to the present day. This will enable us to see, the more clearly, the exact nature of its methods, by showing us the difficulties it has had to encounter, and the precise manner in which it has surmounted them. It will also disclose, in a clear light, the merits of

the various methods of the calculus in the successive stages of its development from Euclid and Archimedes to Cavalieri and Pascal, and from Cavalieri and Pascal to Newton and Leibnitz. Nor is this all. For such a historical sketch will show us that, after all its wanderings through the dark undefined regions of the infinite, the human mind will have to come back to the humble and unpretending postulates of Euclid and Archimedes in order to lay out and construct a satisfactory and easy road across the Alpine heights of the transcendental analysis. And besides, is there not a pleasure —is there not an inexpressible delight in the contemplation of the labors of the human mind by which it has created by far its most sublime instrument of discovery; an instrument, indeed, with which it has brought to light the secrets of almost every department of nature, and with which, above all, it has unveiled the entire system of the material universe to the wonder and admiration of the world?

THE METHOD OF EXHAUSTION.

The ancient geometers, starting with the principle of superposition, were enabled to compare triangles, to ascertain their properties and the measure of their surfaces. From triangles they proceeded to the investigation of polygons, which may be easily divided into triangles, and thence to the consideration of solids bounded by rectilinear figures, such as prisms, pyramids, and polyedrons. Having ascertained the properties of these magnitudes, they were unable to proceed further without the aid of a more powerful or searching method. Hence *the method of exhaustion* was invented and used by them in their more difficult

researches. This opened a new and brilliant career to the ancient geometry. The theory of curved lines and surfaces was partially developed, and the value of the areas and volumes which they contain determined. It has more than a thousand times been asserted, that the method of exhaustion, used by Euclid and Archimedes, "contains the germ of the infinitesimal analysis" of the moderns. But if we would see this truth in a clear light, or comprehend the precise meaning of what is so often and so vaguely asserted, we must recur to the details or elements of the *method of exhaustion.*

As the ancients, says Carnot, "admitted only demonstrations which are perfectly rigorous, they believed they could not permit themselves to consider curves as polygons of a great number of sides; but when they wished to discover the properties of any one of them, they regarded it as the fixed term, which the inscribed and circumscribed polygons continually approached, as nearly as they pleased, in proportion as they augmented the number of their sides. In this way, they exhausted in some sort the space between the polygons and the curves; which, without doubt, caused to be given to this procedure the name of the *method of exhaustion.*" *

This will, perhaps, be more distinctly seen in an example. Suppose, then, that regular polygons of the same number of sides are inscribed in two circles of different sizes. Having established that the polygons are to each other as the squares of their homologous lines, they concluded, by the method of exhaustion, that the circles are to each other as the squares

* Reflexions sur la Métaphysique du Calcul Infinitésimal, p. 138.

of their radii. That is, they supposed the number of the sides of the inscribed polygons to be doubled, and this process to be repeated until their peripheries approached as near as we please to the circumferences of the circles. As the spaces between the polygons and the circles were continually decreasing, it was seen to be gradually exhausted; and hence the name of the method. But although the polygons, by thus continuing to have the number of their sides doubled, might be made to approach the circumscribed circles more nearly than the imagination can conceive, leaving no appreciable difference between them; they would always be to each other as the squares of their homologous sides, or as the squares of the radii of the circumscribed circles. Hence they conjectured, that the circles themselves, so very like the polygons in the last stage of their fulness or roundness, were to each other in the same ratio, or as "the squares of the radii." But it was the object of the ancient geometers, not merely to divine, but to demonstrate. A perfect logical rigor constituted the very essence of their method. Nothing obscure, nothing vague, was admitted either into their premises, or into the structure of their reasoning. Hence their demonstrations absolutely excluded the possibility of doubt or controversy; a character and a charm which, it is to be lamented, the mathematics has so often failed to preserve in the spotless splendor of its primitive purity.

Having divined that any two circles (C and c) are to each other as the squares of their radii (R and r), the ancient geometers proceeded to demonstrate the truth of the proposition. They proved it to be necessarily true by demonstrating every other possible hypothesis

to be false. Thus, said they, if C is not to c as $R^2 : r^2$; then let us suppose that $C : c :: R^2 : r'^2$; r' being any line larger than r. By a process of reasoning, perfectly clear and rigorous, they proved that this supposition led to an absurdity. Then, again, they supposed that $C : c :: R^2 : r''^2$; r'' being less than r; an hypothesis which, in like manner, was shown to lead necessarily to an absurdity. Hence, as the line which entered into the fourth term of the proportion could be neither greater nor less than the radius r, it was concluded to be that radius itself. This process, by which every possible supposition, except the one to be demonstrated, was shown to lead to an absurdity, has always been called the *reductio ad absurdum.* Hence this complex method, used by the ancient geometers in their most difficult researches, has sometimes been called the *reductio ad absurdum,* as well as the *method of exhaustion*—a form of speech, in both cases, in which a part is put for the whole. The *reductio ad absurdum* is, indeed, generally included in the meaning of those who simply speak of the method of exhaustion, and *vice versa.*

By this method the ancients also demonstrated that the volumes of spheres are to each other as the cubes of their radii; that pyramids having the same altitude are to each other as their bases; that a cone is one-third of a cylinder with the same base and the same altitude.

They used it also in regard to curved surfaces. They imagined other surfaces to be inscribed and circumscribed, of which they gradually increased the number of sides and of zones, in such a manner as to continually approximate toward each other, and con-

sequently to close more and more upon the proposed surface. The property of the mean figure was thus indicated or inferred from the known property of the figures which so nearly coincided with it; and this inference, or conjecture, was verified by the *reductio ad absurdum,* which showed that every contrary supposition led infallibly to a contradiction.

It was thus that Archimedes, the Newton of the ancient world, demonstrated that the convex surface of a right cone is equal to the area of the circle which has for a radius the mean proportional between the side of the cone and the radius of the circle of the base; that the total area of the sphere is equal to four great circles; and that the surface of any zone of a sphere is equal to the circumference of a great circle multiplied by the height of the zone. He likewise demonstrated that the volume of a sphere is equal to its surface multiplied by one-third of its radius. Having determined the surface and the volume of the sphere, it was easy to discover their relations to the surface and the volume of the circumscribed cylinder. Accordingly, Archimedes perceived that the surface of a sphere is exactly equal to the convex surface of the circumscribed cylinder; or that it is to the whole surface of the cylinder, including its bases, as 2 to 3; and that the volumes of these two geometrical solids are to each other in the same ratio; two as beautiful discoveries as were ever made by him or by any other man.*

* When Cicero was in Syracuse he sought out the tomb of Archimedes, and, having removed the rubbish beneath which it had long been buried, he found a sphere and circumscribed cylinder engraved on its surface, by which he knew it to be the tomb of the great geometer.

Carnot has well expressed the merits of the method of exhaustion. "That doctrine," says he, "is certainly very beautiful and very precious; it carries with it the character of the most perfect evidence, and does not permit one to lose sight of the object in view; it was the method of invention of the ancients; it is still very useful at the present day, because it exercises the judgment, which it accustoms to the rigor of demonstrations, and because it contains the germ of the infinitesimal analysis. It is true that it exacts an effort of the mind; but is not the power of meditation indispensable to all those who wish to penetrate into a knowledge of the laws of nature, and is it not necessary to acquire this habit early, provided we do not sacrifice to it too much time?"*

Such were its principal advantages, some of which it still enjoys in a far greater degree than the infinitesimal analysis of the moderns. But, on the other hand, it had its disadvantages; it was indirect and tedious, slow and painful in its movements; and, after all, it soon succumbed to the difficulties by which the human mind found itself surrounded. It could not raise even the mind of an Archimedes from questions the most simple to questions more complex, because it had not the *πoῦ στῶ* on which to plant its lever. Truths were waiting on all sides to be discovered, and continued to wait for centuries, until a more powerful instrument of discovery could be invented. Descartes supplied the *πoῦ στῶ*, the *point d'appui*, and Newton, having greatly improved the method of Archimedes, raised the world of mind into unspeakably broader and more beautiful regions of pure thought.

* Reflexions sur la Métaphysique du Calcul Infinitésimal, p. 138.

The method of the ancients, says Carnot, "contains the germ of the infinitesimal analysis" of Newton and Leibnitz. But he nowhere tells us what that germ is, or wherein it consists. It is certainly not to be found in the *reductio ad absurdum*, for this has been banished from the modern analysis. Indeed, it was to get rid of this indirect and tedious process that Newton proposed his improved method. But there are other elements in the method of the ancients: 1. In every case certain variable magnitudes are used as auxiliary quantities, or as the means of comparison between the quantities proposed; and these auxiliary quantities are made to vary in such a manner as to approach more and more nearly the proposed quantities, and, finally, to differ from them as little as one pleases. 2. The variable quantities are never supposed to become equal to the quantities toward which they were made to approach.

Now here we behold the elements of the modern infinitesimal analysis in its most improved and satisfactory form. The constant quantity, toward which the variable is made or conceived to approach as nearly as one pleases, is, in the modern analysis, called "the limit" of that variable. The continually decreasing difference between the variable and its limit, which may be conceived to become as small as one pleases, is, in the same analysis, known as an "indefinitely small quantity." It has no fixed value, and is never supposed to acquire one. Its only property is that it is a variable quantity whose limit is zero. These are the real elements of the modern infinitesimal analysis. If properly developed and applied, the infinitesimal analysis will retain all the wonderful ease and fertility by which it is characterized, without

losing aught of that perfect clearness of evidence which constitutes one of the chief excellences of the ancient method. But, unfortunately, such a development of the infinitesimal analysis has demanded an enduring patience in the pursuit of truth, and a capacity for protracted research and profound meditation which but few mathematicians or philosophers have been pleased to bestow on the subject. Indeed, the true analysis and exposition of the infinitesimal method is, like the creation of that analysis itself, a work for many minds and for more ages than one. Although a Berkeley, a Maclaurin, a Carnot, a D'Alembert, a Cauchy, a Duhamel, and other mathematicians* of the highest order, have done much toward such an exposition of the infinitesimal analysis; yet no one imagines that all its enigmas have been solved or all its unmathematical obscurities removed.

When the true philosophy of the infinitesimal calculus shall appear, it will be seen, not as a metaphysical speculation, but as a demonstrated science. It will put an end to controversy. It will not only cause the calculus to be all over radiant with the clearness of its own evidence, but it will also reflect a new light on the lower branches of the mathematics, by revealing those great and beautiful laws, or principles, which are common to the whole domain of the science, from the first elements of geometry to the last results of

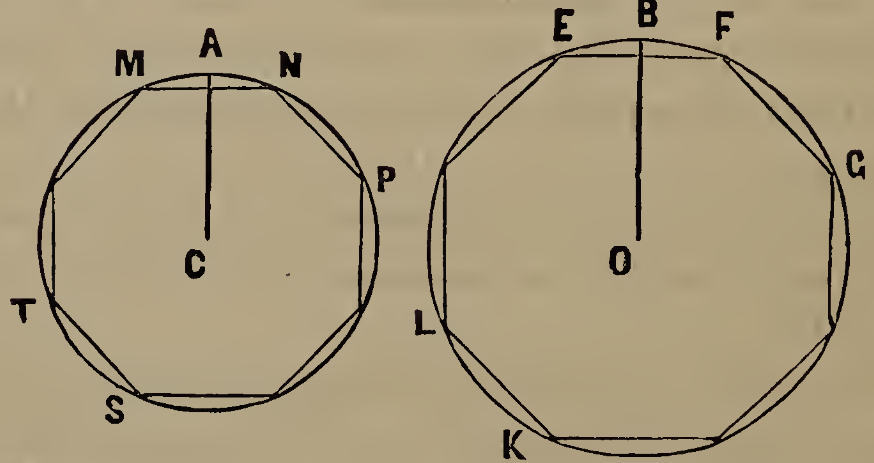
* I have purposely omitted the name of Comte from the above list. Mr. John Stuart Mill has, I am aware, in his work on Logic, ventured to express the opinion that M. Comte "may truly be said to have created the philosophy of the higher mathematics." The truth is, however, that he discusses, with his usual verbosity, "the Philosophy of the Transcendental Analysis," without adding a single notion to those of his predecessors, except a few false ones of his own.

the transcendental analysis. Something of the kind is evidently needed, if we would banish from the elements of geometry the indirect and tedious process of the *reductio ad absurdum*. Accordingly, many attempts have been made, of late, to simplify the demonstrations of Euclid and Archimedes, by introducing the principles of the infinitesimal method into the elements of geometry. But, unfortunately, from a misconception of these principles, they have usually succeeded in bringing down darkness rather than light from the higher into the lower branches of mathematics. Thus, the infinitesimal method, instead of reflecting a new light, is made to introduce a new darkness into the very elements of geometry.

We find, for example, in one of the most extensively used text-books in America, the following demonstration:* "*The circumferences of circles are to each other as their radii, and the areas are as the squares of their radii.*"

Let us designate the circumference of the circle whose radius is C A by *circ.* C A; and its area by *area* C A; it is then to be shown that

circ. C. A : *circ.* O B : : C A : O B, and that
area C A : *area* O B : : C A^2 : O B^2.



* Davies' Legendre, Book V., Proposition XI. Theorem.

Inscribe within the circles two regular polygons of the same number of sides. Then, whatever be the number of sides, their perimeters will be to each other as the radii C A and O B (Prop. X.). Now if the arcs subtending the sides of the polygons be continually bisected until the number of sides of the polygons shall be indefinitely increased, the perimeters of the polygons will become equal to the circumferences of the circumscribed circles (Prop. VIII., Cor. 2), and we have here

circ. C A : *circ.* O B : : C A : O B.

Again, the areas of the inscribed polygons are to each other as C A^2 to O B^2 (Prop. X.). But when the number of sides of the polygons is indefinitely increased, the areas of the polygons become equal to the areas of the circles, each to each (Prop. VIII., Cor. 1); hence we shall have

area C A : *area* O B : : C A^2 : O B^2."

If this were an isolated case, or without any similar demonstrations in the same work, or in other elementary works, it might be permitted to pass without notice. But the principle on which it proceeds forms the basis of the demonstrations of many of the most important propositions in the work before us, and is also most extensively used in other books for the instruction of the young. Hence it becomes necessary to test its accuracy, or its fitness to occupy the position of a first principle, or postulate, in the science of mathematics.

The most scrupulous attention is, in the above instance, paid to all the forms of a demonstration; and

this, no doubt, has an imposing effect on the mind of the beginner. But what shall we say of its substance? The whole demonstration rests on the assumption that an inscribed polygon, with an indefinite number of sides, is equal to the circumscribed circle. Or, in other words, as the author expresses it in a more recent edition of his Geometry, "the circle is but a regular polygon of an infinite number of sides."* The same principle is employed to demonstrate the proposition that "the area of a circle is equal to the product of half its radius by the circumference." Nor is this all. All the most important and beautiful theorems, relating to "the three round bodies," are made to rest on this principle alone; and if this foundation be not valid, then they rest on nothing, except the too easy faith of the teacher and his pupils. One would suppose that if any portion of the science of geometry should have a secure foundation, so as to defy contradiction and silence controversy, it would certainly be the parts above indicated, which constitute the most striking and beautiful features of the whole structure.

In another "Elementary Course of Geometry,"† extensively used as a text-book in our schools and colleges, the same principle is made the foundation of all the same theorems. Indeed, this principle of the "infinitesimal method," as it is called, is even more lavishly used in this last work than in the one already noticed. "The infinitesimal system," says the author, "has been adopted without hesitation, and to an extent somewhat unprecedented. The usual expedients

* Davies' Legendre, revised edition of 1856. Book V., Scholium to Proposition XII.

† Hackley's Geometry.

for avoiding this, result in tedious methods, involving the same principle, only under a more covert form. The idea of the infinite is certainly a simple idea, as natural to the mind as any other, and even an antecedent condition of the idea of the finite."* Now the question before us, at present, relates not to the use of "the infinite" in mathematics, but to the manner in which it is used.

The author tells us that "the perimeter of the polygon of an indefinite number of sides becomes the same thing as the circumference of the circle."† Or, again, "by an infinite approach the polygon and the circle coincide." Now when he informs the student that "the usual expedients for avoiding this" principle "result in tedious methods, involving the same idea only under a more covert form," he certainly requires him to walk by faith, and not by sight or science. It was, as we have said, precisely to avoid the principle that any polygon ever coincides exactly with a circle, that the ancient geometers resorted to the *reductio ad absurdum*, which, from that day to this, has been usually adopted for the purpose of avoiding that principle. "As they admitted only perfectly rigorous demonstrations," says Carnot, as well as every other writer on the subject, "they believed that they could not permit themselves to consider curves as polygons of a great number of sides." Hence they resorted to the indirect and tedious method of demonstration by the *reductio ad absurdum*. This method was, in fact, a protest against the principle in question, a repudiation of it as false and spurious. If the ancient geometers could have adopted that principle, which

* Hackley's Geometry, Preface. † Proposition LXXI.

looked them so fully in the face, they would have been as short and easy in their demonstrations as some modern teachers of the science. But they believed that the fewness of its steps is not the only excellence of a mathematical demonstration. Aiming at a clearness and rigor which would not admit of controversy, they refused to "consider a circle as a polygon of a great number of sides," however great the number. Did they, then, fail to escape the principle in question? Does the *reductio ad absurdum*, their great expedient for avoiding it, really involve that principle? There is certainly not the least appearance of any such thing, and no such thing was ever before suspected. On the contrary, it has hitherto been universally seen and declared that the *reductio ad absurdum* does not involve the principle from which it sought to escape. Yet are we now gravely told, by a distinguished teacher of geometry, "that the usual expedients for avoiding" that principle only "result in tedious methods involving" precisely the same thing! That the *reductio ad absurdum*, the *one* great expedient for this purpose, is, after all, a miserable blunder, involving the very principle from which its authors intended to effect an escape! But if that principle is false, then the weak and tottering foundation of those portions of geometry which it is made to support will require something more than a mere assertion to bolster it up and render it secure.

A third teacher of mathematics and compiler of text-books has, in his "Elements of Geometry," made a similar use of the principle that "a circle is identical with a circumscribed *regular polygon* of an infinite

number of sides."* Now I do not deny that very high authority may be found for this principle, at least among the moderns; but then the foundations of mathematical science rest, not upon authority, but upon its own intrinsic evidence. Indeed, if there had not been high authority for the truth of the principle in question, it is believed that the more humble teachers of geometry would scarcely have ventured to assert it as one of the fundamental assumptions or first principles of the science. It gets rid, it is true, of the tedious and operose *reductio ad absurdum*, and seeks to banish it from the regions of geometry. But will not the stern and unrelenting *reductio ad absurdum* have its revenge on this modern pretender to its ancient honors?

I object to the above so-called principle of "the infinitesimal system," first, because it is obscure. It neither shines in the light of its own evidence, nor in the light of any other principle. That is to say, it is neither intuitively clear and satisfactory to the mind, nor is it a demonstrated truth. Indeed, the authors above referred to do not even pretend to demonstrate it; they merely assume it as a fundamental postulate or first principle. They profess to see, and require their pupils to see, what neither a Euclid nor an Archimedes could clearly comprehend or embrace. Is this because they belong to a more advanced age, and can therefore see more clearly into the first principles of science than the very greatest minds of antiquity? I doubt if much progress has been made

* Elements of Geometry. By James B. Dodd, A. M., Morrison Professor of Mathematics and Natural Philosophy in Transylvania University. Book V., Theorem XXVIII.

since the time of Euclid and Archimedes with respect to the precise relation between a circle and an inscribed or a circumscribed polygon with an infinite number of sides. It is certain that the mathematicians of the present day are not agreed among themselves respecting the truth or the possibility of the conception in question. Thus, for example, one of the teachers of the science rejects the principle in question, "because," says he, "strictly speaking, the circle is not a polygon, and the circumference is not a broken line."* Another teacher of the science says, after having alluded to Euclid, that "modern writers have arrived at many of his conclusions by more simple and concise methods; but in so doing they have, in most instances, sacrificed the rigor of logical demonstration which so justly constitutes the great merit of his writings."† Accordingly, he rejects from the elements of geometry the principle that a circle is a polygon of an infinite number of sides, and returns to the *reductio ad absurdum* of Euclid.

Now, what right have the teachers of geometry to require their pupils to assume as evident a principle which the very masters of the science are utterly unable to receive as true? What right have they to require the mere tyro in geometry to embrace as a first principle what neither a Euclid nor an Archimedes could realize as possible? Even if their principle were true, what right have they to give such strong meat to babes, requiring them to open their mouths,

* Ray's Plane and Solid Geometry, Art. 477.

† Elements of Geometry. By George R. Perkins, A. M., LL.D., Principal and Professor of Mathematics in the New York State Normal School; author of Elementary Arithmetic, Elements of Algebra, etc., etc., etc.

if not to shut their eyes, and implicitly swallow down as wholesome food what the most powerful veterans are so often unable to digest?

The greatest mathematicians and philosophers have, indeed, emphatically condemned the notion that a curve is or can be made up of right lines, however small. Berkeley, the celebrated Bishop of Cloyne, and his great antagonist, Maclaurin, both unite in rejecting this notion as false and untenable. Carnot, D'Alembert, Lagrange, Cauchy, and a host of other illustrious mathematicians, deny that the circumference of a circle, or any other curve, can be identical with the periphery of any polygon whatever. This, then, is not one of the first principles of the science of mathematics. Even if it were true, it would not be entitled to rank as a first principle or postulate, because it admits of doubt, and has, in fact, been doubted and denied in all ages by the most competent thinkers and judges. Whereas, it is the characteristic of all first principles in geometry that they absolutely command the assent of all sane minds, and rivet the chain of inevitable conviction on the universal reason of mankind.

In the second place, I object to the above principle, or rather the above conception, of the infinitesimal analysis, because it is not true. Every polygon is, by its very definition, bounded by a broken line. Now, is the circle bounded by a broken line or by a curve? Every line is that which, according to its definition, has length. How, then, can a right line, which never changes its direction from one of its ends to the other, coincide exactly with a curve line, which always changes its direction? The polygon and the circle are, indeed,

always defined in geometry as distinct and different entities or objects of thought. Why, then, should their definitions be broken up and confounded, as if there were no essential difference between the things defined? Is not this done in the darkness of the imagination rather than in the pure light of reason? If the circle is only one species of the polygon, why not say so in our definitions, and thus carry this first principle down into the very foundations of the science? Why distinguish and then confound them? The truth is, the principle that a curve is made up of indefinitely small right lines is one of those false conceptions of the infinitesimal method which, as we shall hereafter see, have formed themselves into the clouds and darkness that have so long hung around the heights of the transcendental analysis. If, then, we can bring no better principle than this from those heights down into the elements of geometry, had we not just as well bring nothing at all? Here, at least, in the first elements, let all be perfectly clear and incontestable. If we cannot have any principles or laws of light running from the lowest to the highest branches of the mathematics, and binding them all in one, at least let us have no universal laws of darkness.

How the mind of any one, indeed, ever happened to adopt the principle under consideration, and build on it as one of the foundations of mathematical science, is a very curious question, and can be answered only by a careful study of the history of the infinitesimal analysis. When I come to consider that analysis, as developed by Cavalieri, Pascal, Leibnitz, and Newton, I shall return to the examination of this principle and refute it, both by tracing it to its source (which is often

the best way to refute an error), and by showing the contradictions and absurdities in which it is involved.

The most celebrated of the above writers on the elements of geometry does not seem, indeed, to have been long satisfied with his own demonstration. Hence, in a revised edition of his work* the principle in question is not seen, and the word *limit* is substituted in its place. I say the word *limit*, because this term is not adequately defined by him. "The limit of the perimeter" (of the inscribed polygon), says he, "is the circumference of the circle; the limit of the apothem is the radius, and the limit of the area of the polygon is the area of the circle. Passing to the limit, the expression for the area becomes," and so forth. Now what does the author mean by the expression "passing to the limit?" Does he mean that the variable polygon will ultimately become the circle or pass into its limit? If so, then he has made no change whatever in the structure of his former demonstration, except the substitution of an undefined term for an unintelligible principle. Yet he evidently means that the polygon will coincide with the circle; for after saying that "the circumference is the limit of its (variable) perimeter," he adds, that "no sensible error can arise in supposing that what is true of such a polygon is also true of its limit, the circle."† No sensible error! But can any error at all arise? If so, then the polygon does not, strictly speaking, coincide with the circle. But he relieves the student from all hesitation on this point by assuring him, in the next sentence, that "the circle is but a polygon of an infinite number

* Davies' Legendre, revised edition of 1856.

† Book V., Prop. XII., Scholium 2.

of sides." Why, then, attempt to introduce the unnecessary idea of limits? If the polygon really coincides with the circle, or if the circle is only one species of the polygon, then, most assuredly, whatever is true of every regular polygon is also true of the circle. Why, then, introduce the wholly unnecessary notion of a limit? Was this merely to conceal a harsh conception by the use of a hard term?

It is certain that the author did not long continue satisfied with this form of his demonstration; for, in a still later revised edition of his Geometry, he dismisses the notion of limits altogether, and returns still more boldly to the use of "the infinite."* Thus he builds the demonstration of all the same theorems on the principle that "if the number of sides be made infinite, the polygon will coincide with the circle, the perimeter with the circumference, and the apothem with the radius."† Or, more simply expressed, on the idea that "the circle is only a regular polygon of an infinite number of infinitely small sides." But who can see what takes place in the infinite? We are told that two parallel lines meet at infinity, or if produced to an infinite distance. If so, it would be easy to prove that two parallel lines may be perpendicular to each other. We are also told that many other things, equally strange and wonderful, happen at an infinite distance. Hence I hope, for one, that it is the destination of geometry to be rescued from the outer darkness of the infinite and made to shine in the pure, unmixed light of finite reason.

But if the circle is really a regular polygon with an infinite number of sides, then let this be shown

* See edition of 1866. † Book V., Prop. XIV.

once for all, and afterwards proceeded on as an established principle. Why should constructions be continually made in every demonstration, and the same process repeated, only to arrive at the conclusion that a circle has the properties of a regular polygon with an infinite number of sides? Why continue to establish that which is already supposed to be established? If a circle is really "but a regular polygon with an infinite number of sides," then it is evident that the cylinder is only a right prism, and the cone only a right pyramid with such polygons for their bases, and the sphere itself is only a solid generated by the revolution of such a polygon around one of its diameters. Hence all the theorems relating to the circle and the "three round bodies," which are demonstrated in Book VIII. of the work before us, are only special cases of the propositions already demonstrated in regard to the regular polygon, the right cone, and the volume generated by the revolution of a regular polygon around a line joining any two of its opposite vertices. Why, then, after having demonstrated the general truths or propositions, proceed, with like formality, to demonstrate the special cases? Is this conformed to the usage of geometers in other cases of the same kind? Do they prove, first, that the sum of the angles of any triangle is equal to two right angles, and then prove this of the isosceles triangle, or of any other special case of that figure? If not, why prove what is true of all regular polygons whatever, and then demonstrate the same thing in relation to the special case of such a polygon called the circle? The only reason seems to be that although they assume and assert that "a circle is but a regular polygon of an

infinite number of sides," they are not clearly convinced of the truth of this assumption themselves.

If this assumption may be relied on as intuitively certain, or as unquestionably true, then how greatly might the doctrine of the "three round bodies" be simplified and shortened! All the theorems relating to them would, indeed, be at the very most only simple corollaries flowing from propositions already demonstrated. Thus, the volume of the cylinder as a species of the right prism would be equal to its base into its altitude, and its convex surface equal to the periphery of its base into the same line. In like manner the volume of the cone, considered as a right pyramid, would be equal to its base into one-third of its altitude, and its convex surface equal to the periphery of its base into one-half of its slant height. In the same way we might deduce, or rather simply restate, all the theorems in regard to the frustum of a cone, and all those which relate to the sphere. But what, then, would become of Book VIII. of the Elements? Would it not be far too short and simple? As it is, what it lacks in the substance it makes up in the form of its demonstrations. It is now spread, like gold-leaf, over twenty goodly octavo pages; and yet, if the principle on which it is based be really true and satisfactory, the whole book might be easily contained in a few lines, without the least danger of obscurity. Strip the demonstrations of this book, then, of all their needless preparations and forms, and how small the substance! Remove the scaffolding, and how diminutive the edifice! It would scarcely make a decent appearance in the market.

But if we reject the notion that the inscribed regu-

lar polygon ever becomes equal to the circle, or coincides with it, what shall we do? If we deny that they ever coincide, how shall we bridge over the chasm between them, so as to pass from a knowledge of right-lined figures and volumes to that of curves and curved surfaces? Shall we, in order to bridge over this chasm, fall back on the *reductio ad absurdum* of the ancients? or can we find a more short and easy passage without the sacrifice of a perfect logical rigor in the transit? This is the question. This is the very first problem which is and always has been presented to the cultivators of the infinitesimal method. Is there, then, after the lapse and the labor of so many ages, no satisfactory solution of this primary problem? It is certain that none has yet been found which has become general among mathematicians. I believe that such a solution has been given, and that it only requires to be made known in order to be universally received, and become a possession for ever—a *κτῆμα ἐς ἀεί* more precious even than the gift of Thucydides.

But there are mighty obstacles to the diffusion of such knowledge. The first and the greatest of these is the authority of great names; for, as was said more than two thousand years ago, "With so little pains is the investigation of truth pursued by most men, that they rather turn to views already formed." Especially is this so in a case like the present, since the great creators of the calculus, before whom we all bow with the most profound veneration, are very naturally supposed to have known all about the true analysis and exposition of their own creation. But the fact is demonstrably otherwise. Newton himself revealed the secret of the material universe, showing it to be a

fit symbol of the oneness, the wisdom, and the power of its divine Author; but he left the secret of his own creation to be discovered by inferior minds. May we not, then, best show our reverence for Newton, as he showed his for God, by endeavoring, with a free mind, to comprehend and clearly explain the mystery of his creation?

The second of these obstacles is, that few men can be induced to bestow on the subject that calm, patient, and protracted attention which Father Malebranche so beautifully calls "a natural prayer for light." Hence, those who reject the solutions most in vogue usually precipitate themselves and their followers into some false solution of their own. Satisfied with this, although this fails to satisfy others, their investigations are at an end. Henceforth they feel no need of any foreign aid, and consequently the great thinkers of the past and of the present are alike neglected. Their own little taper is the sun of their philosophy. Hence, in their prayerless devotion to truth, all they do is, for the most part, only to add one falsehood more to the empire of darkness. I could easily produce a hundred striking illustrations of the truth of this remark. But with the notice of one in one of the books before me, I shall conclude this first chapter of my reflections.

It is expressly denied in the book referred to that a polygon can ever be made to coincide with a circle. An inscribed polygon, says the author, "can be made to approach as nearly as we please to equality with the circle, *but can never entirely reach it.*" * Accordingly, he defines the limit of a variable in general to be that constant magnitude which the variable can be

* Ray's Plane and Solid Geometry, Art. 475.

made to approach as nearly as we please, but which it "can never quite reach." Now this is perfectly true. For, as the author says, the polygon, so long as it continues a polygon, can never coincide with a circle, since the one is bounded "by a broken line" and the other by "a curve." Here, then, there is a chasm between the inscribed variable polygon and its limit, the circle. How shall this chasm be passed? How shall we, in other words, proceed from a knowledge of the properties of the polygon to those of the circle? The author bridges over, or rather leaps, this chasm by means of a newly-invented axiom. "Whatever is true up to the limit," says he, "is true at the limit."* That is to say, whatever is true of the polygon in all its stages, is true of the circle. Now is not this simply to assume the very thing to be established, or to beg the question? We want to know what is true of the circle, and we are merely told that whatever is always true of the polygon is also true of the circle! In this the author not only appears to beg the question, but also to contradict himself. For, according to his own showing, the polygon is always, or in all its stages, bounded by a broken line, and "the circumference of the circle is not a broken line."† Again, he says that the polygon is always less than the circumscribed circle, and this certainly cannot be said of the circle itself. He appears to be equally unfortunate in other assertions. Thus, he says, "whatever is true of every broken line having its vertices in a curve is true of that curve also."‡ Now the broken line has "vertices" or angular points in the curve; has the circumference of a circle any vertices in it? Again,

* Art. 198. † Art. 477. ‡ Art. 201.

"whatever is true of any secant passing through a point of a curve is true of the tangent at that point."* Now every secant cuts the circumference of the circle in two points, and, as the author demonstrates, the tangent only touches it in one point. Thus, his assumption or universal proposition is so far from being an axiom that it evidently appears not to be true.

The author does not claim the credit of having discovered or invented this new axiom. "In explaining the doctrine of limits," says he, "the axiom stated by Dr. Whewell is given in the words of that eminent scholar."† Now Dr. Whewell certainly had no use whatever for any such axiom. For, according to his view, the variable magnitude not only approaches as nearly as we please, but actually reaches its limit. Thus, says he, "a line or figure *ultimately coincides* with the line or figure which is its limit."‡ Now, most assuredly, if the inscribed polygon ultimately coincides with the circle, then no new axiom is necessary to convince us that whatever is always true of the polygon is also true of the circle. For this is only to say that whatever is true of the variable polygon in all its forms is true of it in its last form — a truism which may surely be seen without the aid of any newly-invented axiom. According to his view, indeed, there was no chasm to be bridged over or spanned, and consequently there was no need of any very great labor to bridge it over or to span it. His axiom was, at best, only a means devised for the purpose of passing over nothing, which might have been done just as well by standing still and doing nothing. The truth

* Art. 201. † Preface. ‡ Doctrine of Limits, Book II., Art. 4.

is, however, that although he said the two figures would ultimately "coincide," leaving no chasm between them to be crossed, he felt that there would be one, and hence the new axiom for the purpose of bridging it over. But the man who can adopt such a solution of the difficulty, and, by the authority of his name, induce others to follow his example, only interposes an obstacle to the progress of true light and knowledge. Indeed, the attempts of Dr. Whewell to solve the enigmas of the calculus are, as we shall have occasion to see, singularly awkward and unfortunate; showing that the depth and accuracy of his knowledge are not always as wonderful as its vast extent and variety.

CHAPTER II.

DEFINITION OF THE FIRST PRINCIPLES OF THE INFINITESIMAL METHOD.

In the preceding chapter it has been shown that it is an error to consider a circle as a polygon. It is certainly a false step to assume this identity, in any case, as a first principle or postulate, since so many mathematicians of the highest rank regard it as evidently untrue. Thus Carnot, for example, says, "It is absolutely impossible that a circle can ever be considered as a true polygon, whatever may be the number of its sides."* The same position is, with equal emphasis, assumed by Berkeley, Maclaurin, Euler, D'Alembert, Lagrange, and a host of other eminent mathematicians, as might easily be shown, if necessary, by an articulate reference to their writings. But, indeed, no authority is necessary either to establish or to refute a first principle or postulate in geometry. This is simply a demand upon our reason which is only supported by assertion, and put forth either to be affirmed or denied. If the reason of mathematicians does not affirm it, then is there an end of its existence as a first principle or postulate. As no effort is made to prove it, so none need be made to refute it. For no one has a right to be heard in geometry who makes the science start from unknown or contradicted

* Reflexions, etc., chapter I., p. 11.

principles, especially from such principles as have, in all ages, been rejected by the mathematicians of the very highest order. Yet has there been, in modern times, an eager multitude of geometers who rush in where a Euclid and an Archimedes feared to tread. Let us see, then, if we may not find a safer and more satisfactory road to the same result.

The problem to be solved is, as we have seen, how to pass from the properties of rectilinear figures to those of curvilinear ones. Or, in particular, how to pass from the known properties of the polygon to a knowledge of the properties of the circle. Since no polygon can, *ex hypothesi*, be found which exactly coincides with the circle, we are not at liberty to transfer its properties to the circle, as if it were a polygon with a great number of sides. For, having inscribed a regular polygon in a circle, and bisected the arcs subtended by its sides, we may double the number of its sides, and continue to repeat the process *ad libitum;* and yet, according to hypothesis, it will never exactly coincide with the circumscribed circle. There will, after all, remain a chasm between the two figures—between the known and the unknown. Now the question is, how to bridge over this chasm with a perfectly rigorous logic in order that we may clearly, directly, and expeditiously pass from the one side to the other, or from the known to the unknown? The method of limits affords a perfect solution of this question. Nor is this all. For, in the clear and satisfactory solution of this problem, the very first relating to the infinitesimal analysis, it opens, as we shall be enabled to see, a vista into one of the most beautiful regions of science ever discovered by the genius of man. Let

us, then, proceed to lay down the first principles of this method, and produce the solution of the above problem.

The limit of a variable.—When one magnitude takes successively values which approach more and more that of a constant magnitude, and in such manner that its difference from this last may become less than any assigned magnitude of the same species, we say that the first *approaches indefinitely the second*, and that the second is its *limit*.

Thus, the *limit* of a variable is *the constant quantity which the variable indefinitely approaches, but never reaches.**

"The importance of the notion of a *limit*," says Mr. Todhunter, "cannot be over-estimated; in fact, the whole of the differential calculus consists in tracing the consequences which follow from that notion."† Now this is perfectly true. Duhamel says precisely the same thing. But, then, the consequences of this notion or idea may be traced clearly, and every step exhibited as in the open light of day; or they may be traced obscurely, and almost the whole process concealed from the mind of the student behind an impenetrable veil of symbols and formulæ. They may be shown to flow, by a perfectly clear and rigorous course of reasoning, from the fundamental definition or idea of the infinitesimal method, or they may be deduced from it by a process which looks more like legerdemain than logic. In this respect there appears to be a vast difference between the above-named

* Eléments de Calcul Infinitésimal, par M. Duhamel, Vol. I., Book I., chap. I., p. 9.

† Dif. and Int. Calculus, p. 4.

mathematicians. The student who follows the guidance of the one sees everything about him, and is at every step refreshed and invigorated by the pleasing prospects presented to his mind. On the contrary, the student who pursues the analysis of the other resembles, for the most part, the condition of a man who feels his way in the dark, or consents to be led blindfold by a string in the hand of his guide.

The very first point of divergence in these two very different modes of development is to be found in the definition of the all-important term *limit*. In the definition of M. Duhamel, the variable is said not to reach its *limit*, while in that of Mr. Todhunter this element of the "notion of a limit" is rejected. "The following may," says he, "be given as a definition: The limit of a function (or dependent variable) for an assigned value of the independent variable, *is that value from which the function can be made to differ as little as we please by making the independent variable approach its assigned value.*" * There is, in this definition, not a word as to whether the variable is supposed to reach its limit or otherwise. But the author adds, "Sometimes in the definition of a limit the words 'that value which the function never actually attains' have been introduced. But it is more convenient to omit them." Now this difference in the definition of a limit may, at first view, appear very trifling, yet in reality it is one of vast importance. If, at the outset of such inquiries, we diverge but ever so little from the strict line of truth, we may ultimately find ourselves involved in darkness and confusion. Hence, it is necessary to examine this difference of definition, and,

* Chapter I., p. 6.

if possible, ascertain which of the two guides we should follow.

Is the definition of a *limit*, then, of the one all-important idea of the infinitesimal calculus, a mere matter of convenience, or should it be conformed to the nature of things? The variables in the calculus are always subjected to certain conditions or laws of change, and in changing according to those conditions or laws they either reach their limits or they do not. If they do reach them, then let this fact be stated in the definition and rigidly adhered to without wavering or vacillation. Especially let this be done if, as in the work before us, the same fact is everywhere assumed as unquestionably true. Thus, the limit of a variable is supposed to be its "limiting value,"* or the last value of that variable itself. Again, he still more explicitly says, "any *actual value* of a function may be considered as a limiting value."† Having assumed that the variable actually reaches its limit, it would, indeed, have been most inconvenient to assert, in his definition, that it never reaches it; for this would have been to make one of his hypotheses contradict the other. But if it be a fact that the variable does reach its limit, and if this fact be assumed as true, then why not state it in the definition of a limit?

The reason is plain. This, also, would have been very inconvenient, since the author would have found it very difficult to verify the correctness of his definition by producing any variables belonging to the infinitesimal analysis that actually reach their limits. He might easily find lawless variables, or such as occur to the imagination while viewing things in the

* Chapter I., p. 6. † Ibid.

abstract, which may reach their limits. But such variables are not used as auxiliary quantities in the infinitesimal analysis. They would be worse than useless in all the investigations of that analysis. Hence, if he would verify his assumption, he must produce variables of some use in the calculus which are seen and known to reach their limits. Can he produce any such variables? He has certainly failed to produce even one.

In order to illustrate his "notion of a limit," he adduces the geometrical progression $1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} +$, etc. Now, as he truly says, "the limit of the sum of this series, when the number of terms is indefinitely increased, is 2." But does this sum actually reach its limit 2? Or, in other words, if we continue to make each term equal to one-half of the preceding term, shall we ever reach a term equal to nothing? Or, in other words again, is the half of something ever nothing? If so, then two nothings may be equal to something, and, after all, the indivisibles of Cavalieri was no mathematical or metaphysical dream. If we may divide a quantity until it ceases to have halves, or until one-half becomes absolutely nothing, then have the mathematical world greatly erred in rejecting these indivisibles as absurd, and we may still say that a line is equal to the sum of an indefinite number of points, a surface to an indefinite number of lines, and a volume to an indefinite number of surfaces. But is not the mathematical world right? Is it not a little difficult to believe that the half of something is nothing? Or that a line which has length may be so short that its half will be a point or no length at all? Be this as it may, the infinite divisibility of magnitude, as well as

the opposite doctrine, may be a metaphysical puzzle; but it has no right to a place in mathematics, much less to the rank of a fundamental assumption or postulate. But it must be regarded as such if we may assert that the sum of the progression $1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} +$, etc., actually reaches its limit 2 by being sufficiently far produced. We shall certainly escape such dark and darkening assumptions if we can only find a method for passing, in the order of our knowledge, from the variable to its limit without supposing the variable itself to pass to its limit. Precisely such a method we have in the work of Duhamel, and nothing approximating to it in the differential calculus of the English mathematician.*

Our author gives another illustration of the idea of a limit. "Although $\dfrac{\sin\theta}{\theta}$ approaches as nearly as we please to the limit, *it never actually attains that limit.*" † Both the words and the italics are his own. Here it is said that the variable "never actually attains its limit," and this, I apprehend, will be found to be the case in relation to every variable really used in the infinitesimal method. It will, at least, be time enough to depart from the definition of Duhamel when variables are produced from the calculus which are seen to reach their limits without violating the law of their

* It has often been a subject of amazement to my mind that the English mathematicians derive so little benefit from the improvements introduced by their French neighbors. Why, in the republic of letters and science, should there not be a free interchange of ideas and improvements? The French were not slow to borrow the methods of Newton; but the English seem exceedingly slow, if not disinclined, to borrow from a Carnot, a Cauchy, or a Duhamel the improvements which they have made in these methods.

† Chapter I., p. 6.

increase or decrease. If such variables should be found, then, since some are admitted to exist which never reach their limits, such quantities should be divided into two classes and discussed separately. That is to say, the analyst should then treat of those variables which reach their limits and of those which never reach their limits. But it is to be hoped that he will cease to take any further notice of the first class of variables until some such can be found that are capable of being used in the calculus.

Let us return to the original instance of the circle and the polygon, because this will make the idea perfectly plain. Duhamel knows, as Euclid demonstrated, that such a variable polygon may be made to approach the dimensions of the circle as nearly as one pleases. He knows this, indeed, just as well as he knows any property of the polygon itself, or of any other figure in geometry. He takes his stand, then, upon the demonstrated truth that the difference between the dimensions, or the areas, of the two figures may be made less than any "grandeur designée," than any assigned magnitude of the same species. This knowledge, this clearly perceived, this demonstrated truth, is the point from which he sets out to bridge the chasm between the one figure and the other. He never supposes the two figures to coincide or to become equal, because he has the means of spanning the chasm which separates them without either denying its existence or filling it up with doubtful propositions about what may be supposed to take place at the end of an infinite process. He has no use for any such assumptions or assertions even if true, because he has a much clearer and better method to obtain the same

result. But before we can unfold that method in a clear and perspicuous manner it will be necessary to consider his next definition.

"*We call,*" says he, "*an infinitely small quantity, or simply an infinitesimal, every variable magnitude of which the limit is zero.*

"For example, the difference between any variable whatever, and its limit, is said to be infinitely small, since it tends towards zero. Thus the difference of the area of a circle from that of the regular inscribed polygon of which the number of sides is indefinitely multiplied, is infinitely small. It is the same with the difference between a cylinder and an inscribed prism, or a cone and the inscribed pyramid, etc., etc.

"We cite these particular cases in order to indicate some examples, but infinitely small quantities may present themselves in a multitude of circumstances where they are not differences between variables and their limits."

It is to be regretted, perhaps, that Duhamel did not use the term "*infinitesimal*" instead of the more ambiguous words "*infinitely small,*" in order to express the idea which he has so clearly defined. There is, however, nothing obscure in his meaning. An infinitely small quantity is, as he defines it, not a fixed or constant quantity at all, much less one absolutely small, or one beyond which there can be no smaller quantity. It is, on the contrary, always a variable quantity, and one which has zero for its limit. Or, according to his definition of a limit, an infinitesimal is a variable which may be made to approach as near to zero as one pleases, or so near as to reduce its difference from zero to less than any assigned

quantity. Thus, it never becomes infinitely small, in the literal sense of the terms, or so small that it cannot be made still smaller. It is, on the contrary, its distinguishing characteristic that it may become smaller and smaller without ever acquiring any fixed value, and without actually reaching its limit, zero. It is from these two ideas of a *limit* and an *infinitesimal*, says he, that the whole system of truths contained in the calculus flows in the form of logical consequences. But in order to develop these ideas, or apply them to the investigation of truth, he found it necessary to establish

THE FUNDAMENTAL PRINCIPLE OF LIMITS.

It is precisely for the want of this principle, and a knowledge of its applications, that so many mathematicians, both in England and America, have discussed the processes of the differential calculus in so obscure and unsatisfactory a manner. This principle is indispensable to render the lamp of the infinitesimal analysis a sufficient light for our eyes, as well as guide for our feet. This principle is as follows:—

"*If two variable quantities are constantly equal and tend each toward a limit, these two limits are necessarily equal.*—In fact, two quantities always equal present only one value, and it seems useless to demonstrate that one variable value cannot tend at the same time towards two unequal limits, that is, towards two constant quantities different from one another. It is, moreover, very easy to add some illustrations which render still clearer, if possible, this important proposition. Let us suppose, indeed, that two variables always equal have different limits, A and B; A being,

for example, the greatest, and surpassing B by a determinate quantity Δ. The first variable having A for a limit will end by remaining constantly comprised between two values, one greater the other less than A, and having as little difference from A as you please; let us suppose, for instance, this difference less than $\frac{1}{2}\Delta$. Likewise the second variable will end by remaining at a distance from B less than $\frac{1}{2}\Delta$. Now it is evident that then the two values could no longer be equal, which they ought to be, according to the data of the question. These data are, then, incompatible with the existence of any difference whatever between the limits of the variables. Then these limits are equal.

The following principle is more general and more useful than that laid down by Duhamel, and, besides, it admits of a rigorous demonstration:

If, while tending toward their respective limits, two variable quantities are always in the same ratio to each other, their limits will be to one another in the same ratio as the variables.

Let the lines A B and A C represent the limits of any two variable magnitudes which are always in the same ratio to one another, and let Ab, Ac represent

A b c B′ b′ B C′ c′ C C″

two corresponding values of the variables themselves; then Ab : Ac : : A B : A C.

If not, then Ab : Ac : : A B : some line greater or less than A C. Suppose, in the first place, that Ab : Ac : : A B : A C′; A C′ being less than A C. By hypothesis, the variable Ac continually approaches A C, and may be made to differ from it by less than

any given quantity. Let Ab and Ac, then, continue to increase, always remaining in the same ratio to one another till Ac differs from A C by less than the quantity C′ C; or, in other words, till the point c passes the point C′, and reaches some point, as c′, between C′ and C, and b reaches the corresponding point b′. Then, since the ratio of the two variables is always the same, we have

$$Ab : Ac :: Ab' : Ac'.$$

By hypothesis, $Ab : Ac :: AB : AC'$;

hence $Ab' : Ac' :: AB : AC'$,

or $AC' \times Ab' = Ac' \times AB$;

which is impossible, since each factor of the first member is less than the corresponding factor of the second member. Hence the supposition that Ab : Ac :: A B : A C′, or to any quantity less than A C, is absurd.

Suppose, then, in the second place, that Ab : Ac :: A B : A C″, or to some term greater than A C. Now there is some line, as A B′, less than A B, which is to A C as A B is to A C″. If, then, we conceive this ratio to be substituted for that of A B to A C″, we have

$$Ab : Ac :: AB' : AC;$$

which, by a process of reasoning similar to the above, may be shown to be absurd. Hence, if the fourth term of the proportion can be neither greater nor less than A C, it must be equal to A C; or we must have

$$Ab : Ac :: AB : AC. \qquad \text{Q. E. D.}$$

Cor. *If two variables are always equal, their limits are equal.*

The above truth is, as has already been said, the great fundamental principle of the infinitesimal analysis, which, being demonstrated once for all by the rigorous method of the *reductio ad absurdum,* will easily help us over a hundred chasms lying between rectilinear and curvilinear figures, as well as between volumes bounded by plane surfaces and those bounded by curved surfaces, and introduce us into the beautiful world of ideas beyond those chasms. But before we can apply this prolific principle to the solution of problems or to the demonstration of theorems, it will be necessary to establish one or two preliminary propositions. These are demonstrated by Duhamel as follows:

1. *The limit of the sum of the variables $x, y, z \ldots u$, of any finite number whatever which have respectively for their limits $a, b, c \ldots l$, positive or negative, is the algebraic sum of those limits.* In fact, the variables $\mathrm{x, y, z} \ldots \mathrm{u}$ can be represented by $\mathrm{a} + \alpha$, $\mathrm{b} + \beta, \ldots \mathrm{l} + \lambda$, the differences $\alpha, \beta, \ldots \lambda$ having each zero for its limit. We have then $\mathrm{x} + \mathrm{y} + \mathrm{z} + \ldots + \mathrm{u} = (\mathrm{a} + \mathrm{b} + \mathrm{c} + \ldots \mathrm{l}) + (\alpha + \beta + \ldots + \lambda)$. But $\alpha + \beta + \ldots \lambda$ tends towards the limit zero, since it is thus with each of the terms in any finite number which composes that quantity. Then the limit of the second member, and consequently of the first, which is always equal to it, is $\mathrm{a} + \mathrm{b} + \mathrm{c} + \ldots + \mathrm{l}$, which was to be demonstrated.

2. *The limit of the product of several variables is the product of their limits.*—In fact, if we employ the same denomination as in the preceding case, we shall have

$x\,y\,z \ldots u = (a + \alpha)\,(b + \beta +)\,(c + \gamma) \ldots (l + \lambda) = a\,b\,c \ldots l + \omega$, ω designating the sum of a finite number of terms, each having zero for its limit, since they contain as factors at least one of the quantities $\alpha, \beta, \gamma \ldots \lambda$, each of which has zero for its limit. We see, then, that the second member, or the first $x\,y\,z \ldots u$, has for its limit $a\,b\,c \ldots l$, which was to be demonstrated.

3. *The limit of the quotient of two variables is the quotient of their limits.*—In fact,

$$\frac{x}{y} = \frac{a + \alpha}{b + \beta} = \frac{a}{b} + \frac{b\alpha - a\beta}{b\,(b + \beta)}.$$

But the denominator of the last fraction can be made as nearly as we please equal to b^2, which is a constant quantity different from zero; its numerator tends towards zero; then the fraction has zero for its limit. The limit of $\frac{x}{y}$ is then $\frac{a}{b}$, the proposition to be demonstrated.

4. *The limit of a power of a variable is the same power of its limit.*—For, supposing the degree of its power to be the entire number, m, then x^m is the product of m factors equal to x, and, according to the case 2, the limit of x^m will be a^m.

Let $m = \frac{p}{q}$, p and q being any entire numbers whatever; $x^{\frac{p}{q}}$ is the power p of $x^{\frac{1}{q}}$; then, according to the preceding case, the limit of $x^{\frac{p}{q}}$ is the p power of the limit $x^{\frac{1}{q}}$ or of $\sqrt[q]{x}$; it remains to find this last. But x, being the product of q factors equal to $\sqrt[q]{x}$, has for its limit the q power of the limit of $\sqrt[q]{x}$, and as x has for

its limit a, it follows that a is the q power of the limit of $\sqrt[q]{x}$, or that $\sqrt[q]{x}$ has for a limit $\sqrt[q]{a}$. Then $x^{\frac{p}{q}}$ has for a limit $a^{\frac{p}{q}}$, as we have enunciated.

These principles will be found exceedingly easy in practice, as well as clear and rapid in arriving at the most beautiful results. I shall begin with cases the most simple, and proceed with equal ease and clearness to solve problems and prove theorems which are usually esteemed more difficult.

APPLICATION TO SIMPLE QUESTIONS IN THE ELEMENTS OF GEOMETRY.

1. *The surfaces of any two circles are to each other as the squares of their radii.*

Let S, S′ be the surfaces of any two circles, and R, R′ their radii. These surfaces, we know, are the limits of two regular inscribed polygons, whose sides, always equal in number, are supposed to be doubled an indefinite number of times. But these polygons are always to each other as the squares of the radii of the circumscribed circles. Hence their limits, the circles themselves, are to each other in the same ratio. That is,

$$S : S' :: R^2 : R'^2,$$

which is the proposition to be demonstrated.

2. *The circumferences of any two circles are to each other as their radii.*

Let the inscribed auxiliary polygons be as in the last case. The circumferences of the circles are then the limits of the peripheries of the polygons. But these peripheries are to each other as the radii of the

circumscribed circles. Hence their limits, the circumferences, are in the same ratio to each other. That is, if C, C′ be circumferences, we shall have

$$C : C' :: R : R',$$

the proposition to be demonstrated.

3. *The area of a circle is equal to half its circumference into its radius.*

Let P denote the inscribed auxiliary polygon, *a* its apothem, and *p* its periphery. Then we shall always have

$$P = \tfrac{1}{2}\, a, p.$$

But if two variables are always equal, their limits will be equal. Hence

$$S = \tfrac{1}{2}\, R\ C,$$

since the limit of P is S, and the limit of the product $\frac{1}{2}$ a, p is the product of the limits $\frac{1}{2}$ R C. Q. E. D.

4. *The volume of a cone is equal to the product of its base by one-third of its altitude.*

The cone is the limit of a pyramid having the same vertex, and for its base a polygon inscribed in the base of the cone, of which the number of sides may be indefinitely increased. Let V be the volume of the cone, B its base, and H its height, and let V′, B′ be the volume and the base of the inscribed pyramid, whose height is also H; since every pyramid is measured by one-third of its base into its height, we have

$$V' = \tfrac{1}{3}\, B'\ H.$$

But if two variables are always equal, their limits are equal. Hence

$$V = \tfrac{1}{3} B, H,$$

which proves the proposition as enunciated.

By the application of the above principle, that if any two variables have an invariable ratio to each other, then their limits will necessarily be in the same ratio to each other, the student may easily demonstrate other theorems in the elements of geometry. He may easily prove, for example, that the convex surface of the cone is equal to the circumference of its base into half its slant height; that the volume of a cylinder is equal to its base into its height, and that its convex surface is equal to the circumference of its base into its height; that the volume of a sphere is equal to its surface into one-third of its radius, and that its surface is equal to four great circles. In like manner, he may easily find the measure for the volume and the convex surface of the frustum of a cone, by considering them as the limits of the volume and of the convex surface of the inscribed frustum of a pyramid. Nay, he may go back and by the use of the same method easily find the area of any triangle and the volume of any pyramid.

Nor is this all. For, after having demonstrated in a clear and easy way the theorems in the elements of geometry, the fundamental principle of limits, as above conceived, carries its light into analytical geometry and into the transcendental analysis. It is, indeed, a stream of light which comes down from that analysis, properly understood, and irradiates the lower branches of mathematical science, somewhat as the sun illuminates the planets. If the student will only familiarize his mind with that principle and its appli-

cations, he will find it one of the most fruitful and comprehensive conceptions that ever emanated from the brain of man. At the end of the next chapter but one, we shall see some of its most beautiful applications to the quadrature of surfaces and to the cubature of volumes.

CHAPTER III.

THE METHOD OF INDIVISIBLES.

KEPLER introduced the consideration of infinitely great and infinitely small quantities into the science of mathematics. Cutting loose from the cautious and humble method of the ancients, which seemed to feel its way along the shores of truth, this enterprising and sublime genius boldly launched into the boundless ocean of the infinite. His example was contagious. Others entered on the same dark and perilous voyage of discovery, and that, too, without chart or compass. *Cavalieri* was the first to use such quantities systematically, or to lay down rules for the guidance of the mind in dealing with them. The manner in which he employed them is known as "The Method of Indivisibles," which, it is well known, opened a new and successful career to geometry. He has invariably, and with perfect justice, been regarded as the precursor of those great men to whom we owe the infinitesimal analysis.* The study of his method is, indeed, a necessary prerequisite to a knowledge of the rise, the nature, the difficulties, and the fundamental principles of that analysis.

In the method of indivisibles lines are considered as composed of points, surfaces as composed of lines, and volumes as composed of surfaces. "These hypo-

* Carnot on the Infinitesimal Analysis, chap. III., p. 141.

theses," says Carnot, "are certainly absurd, and they ought to be employed with circumspection."* Now here the question very naturally arises, in every reflecting mind, If these hypotheses or postulates are absurd, why employ them at all? The only answer that has ever been returned to this question is, that such hypotheses should be employed because they lead to true results. Thus, says Carnot, "It is necessary to regard them as means of abbreviation, by means of which we obtain promptly and easily, in many cases, what could be discovered only by long and painful processes according to the method of exhaustion." This method is, then, recommended solely on the ground of its results. We do not and cannot see the justness of its first principles; but still we must accept them as true, because they lead to correct conclusions. That is to say, we must invert the logical order of our ideas and judge of our principles by the conclusions, not of the conclusions by our principles. Nay, however absurd they may appear in the eye of reason, we must, in the grand march of discovery, ask no questions, but just shut our eyes and swallow them down! All honor to *Cavalieri*, and to every man that makes discoveries! But as there is a time for the making of discoveries, so is there also a time for seeing how discoveries are made.

We are told, for example, that a line is made up of points, and, at the same time, that a point has absolutely no length whatever. How many nothings, then, does it take to make something? Who can tell us? The demand is too much for the human mind. The hypothesis is admitted to be absurd, and yet its harsh-

* Carnot on the Infinitesimal Analysis, chap. III., p. 141.

ness is sought to be softened by the assurance that it should be regarded merely as an abbreviation. An abbreviation of what? If it is the abbreviation of any true principle, then it is not absurd at all, since it should evidently be understood to mean the principle of which it is the abridged form or expression. But if it is not an abbreviation of any such principle, then we do not see how our condition is bettered by the use of a big word. This apology for the so-called first principles of the method of infinites has, indeed, been made and kept up from Carnot to Todhunter; but we have not been informed, nor are we able to discover, of what these hypotheses are the abbreviations. If they are abridgments at all, they may be, for aught we can see, abridgments of conceptions as "certainly absurd" as themselves.

After giving one or two beautiful applications of the method of indivisibles, Carnot says: "Cavalieri well asserted that his method is nothing but a corollary from the method of exhaustion; but he acknowledged that he knew not how to give a rigorous demonstration of it." This is true. Cavalieri did not know how to demonstrate his own method, because he did not understand it. He understood it practically, but not theoretically. That is to say, he knew how to apply it so as to make discoveries. But how or why his method happened to turn out true results he did not know, and consequently he could not explain to others. His disciples had to walk by faith and not by science; but if the road was dark, the goal was beautiful. Some of his disciples even eclipsed the master in the beauty and the value of their discoveries. But, after all, their knowledge of the method was

only practical, and consequently they wisely abstained, as a general thing, from attempts to elucidate the principles and the working of its interior mechanism. "The great geometers who followed this method," as Carnot well says, "soon seized its spirit; it was in great vogue with them until the discovery of the new calculus, and they paid no more attention to the objections which were then raised against it than the Bernouillis paid to those which were afterwards raised against the infinitesimal analysis. It was to this method of indivisibles that Pascal and Roberval owed their profound researches concerning the cycloid."* Thus, while appealing to the practical judgment of mankind, they treated the demands of our rational nature with disdain, and the more so, perhaps, because these demands were not altogether silent in their own breasts. A man may, indeed, be well satisfied with his watch, because it truly points to the hour of the day. But when, as a rational being, he seeks to know how this admirable result is brought to pass, is it not simply a grand imposition to turn him off with the assurance that his watch keeps the time? Does this advance his knowledge? Does this enable him to make or to improve watches? Nay, does this even give him the idea of a watch, by showing him the internal mechanism and arrangement of the parts which serve to indicate on its surface as it passes each flying moment of time? No one, says Bishop Butler, can have "the idea of a watch" without such a knowledge of its internal mechanism, or the adaptation of its several parts to one another and to the end which it accomplishes. May we not, then, with equal truth, say that

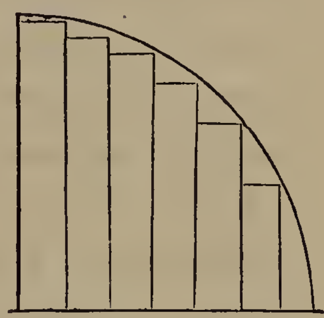
* Chapter III., p. 144.

no one has "the idea" of the method of indivisibles, or of the infinitesimal calculus, unless he can tell by what means and how it achieves its beautiful results? Without such knowledge the mathematician may, it is true, be able to name his tools and to work with them; but does he understand them? Does he comprehend the method he employs?

Blaise Pascal himself, though universally recognized as one of the greatest geniuses that ever lived, could not comprehend the hypotheses or postulates of the method of indivisibles as laid down by Cavalieri. Hence, while he continued to use the language of Cavalieri, he attached a different meaning to it—a change which is supposed by writers on the history of mathematics to have improved the rational basis of the method. By "an indefinite number of lines," said he, "he always meant an indefinite number of small rectangles," of which "the sum is certainly a plane." In like manner, by the term "surfaces," he meant "indefinitely small solids," the sum of which would surely make a solid. Thus, he concludes, if we understand in this sense the expressions "*the sum of the lines, the sum of the planes*, etc., they have nothing in them but what is perfectly conformed to pure geometry." This is true. The sum of little planes is certainly a plane, and the sum of little solids is as clearly a solid. But, from this point of view, it seems improper to call it "the method of indivisibles," since every plane, as well as every solid, may easily be conceived to be *divided*. The improved postulates of Pascal deliver us, indeed, from the chief difficulty of the method of indivisibles, properly so called, only to plunge us into another—into the very one, in fact,

from which Cavalieri sought to effect an escape by the invention of his method.

Let me explain. If we divide any curvilinear figure into rectangles, no matter how small, the sum of these rectangles will not be exactly equal to the area of the figure. On the contrary, this sum will differ from that area by a surface equal to the sum of all the little mixtilinear figures at the ends of the rectangles.



It is evident, however, that the smaller the rectangles are made, or the greater their number becomes, the less will be the difference in question. But how could Cavalieri imagine that this difference would ever become absolutely nothing so long as the inscribed rectangles continue to be surfaces? Hence, in order to get rid of this difference altogether, and to arrive at the exact area of the proposed figure, he conceived the small rectangles to increase in number until they dwindled into veritable lines. The sum of these lines he supposed would be equal to the area of the figure in question; and he was confirmed in this hypothesis, because it was found to conduct to perfectly exact results. Thus, his hypothesis was adopted by him, not because it had appeared at first, or in itself considered, as intuitively certain, but because it appeared to be the only means of escape from a false hypothesis, and because it led to so many exactly true results. But when this hypothesis, abstractly considered, was found to shock the reason of mankind, which, in the words of Carnot, pronounced it "certainly absurd," the advocates of the method of indivisibles were obliged to assume new

ground. Accordingly, they discovered that indivisibles might be divided, and that by "the sum of right lines" was only meant "the sum of indefinitely small rectangles." Pascal seems to believe, in fact, that such was the meaning of Cavalieri himself. It is certain that history has decided otherwise, and delivered the verdict that by indivisibles Cavalieri really meant indivisibles.

Now, it seems just as evident that a curvilinear figure is not composed of rectangles, as that it is not composed of right lines. Yet Pascal, the great disciple, adopted this supposition as the only apparent means of escape from the absurdity imputed to that of the master, and he pointed to the perfect accuracy of his conclusions as a proof of the truth of his hypothesis. For, strange to say, the sum of the rectangles, as well as the sum of the lines, was found to be exactly equal to the area of the curvilinear figure. What, then, became of the little mixtilinear figures at the extremities of the rectangles? How, since they were omitted or thrown out, could the remaining portion of the surface or the sum of the rectangles alone be equal to the whole? Pascal just cut the Gordian knot of this difficulty by declaring that if two finite quantities "differ from each other by an indefinitely small quantity," then "the one may be taken for the other without making the slightest difference in the result." Or, in other words, that an infinitely small quantity may be added to or subtracted from a finite quantity without making the least change in its magnitude. It was on this principle "that he neglected without scruple," as Carnot says, "these little quantities as compared with finite quantities; for we see that

Pascal regarded as simple rectangles the trapeziums or little portions of the area of the curve comprised between two consecutive co-ordinates, neglecting consequently the little mixtilinear triangles which have for their bases the differences of those ordinates."*

Carnot adds, as if he intended to justify this procedure, that "no person, however, has been tempted to reproach Pascal with a want of severity." This seems the more unaccountable, because Carnot himself has repeatedly said that it is an error to throw out such quantities as nothing. Nor is this all. No one can look the principle fairly and fully in the face, that an infinitely small quantity may be substracted from a finite quantity without making even an infinitely small difference in its value, and yet regard it as otherwise than absurd. It is when such a principle is recommended to the mathematician by the desperate exigencies of a system which strains his reason, warps his judgment, and clouds his imagination, that it is admitted to a resting-place in his mind. It was thus, as we have seen, that Pascal was led to adopt the principle in question; and it was thus, as we shall see, that Leibnitz was induced to assume the same absurd principle as an unquestionable axiom in geometry.

Now if, with Cavalieri, we suppose a surface to be composed of lines, or a line of points, then we shall have to add points or no-magnitudes together until we make magnitudes. Nay, if lines are composed of points, surfaces of lines, and solids of surfaces, then is it perfectly evident that solids are made up of points, and the very largest magnitude is composed of that which has no magnitude! Or, in other words, every

* Carnot, chapter III., p. 146.

magnitude is only the sum of nothings! On the other hand, if we agree with Pascal that a curvilinear space is, strictly speaking, composed of rectangles alone, then we shall have to conclude that one quantity may be taken from another without diminishing its value! Which term of the alternative shall we adopt? On which horn of the dilemma shall we choose to be impaled? Any one is at liberty to select that which is the most agreeable to his reason or imagination. But is it, indeed, absolutely necessary to be swamped amid the zeros of Cavalieri or else to wear the yoke of Pascal's axiom? May we not, on the contrary, guided by the careful insight of some new Spallanzani, safely sail between this Scylla and Charybdis of the infinitesimal method? The reader will soon be enabled to answer this question for himself.

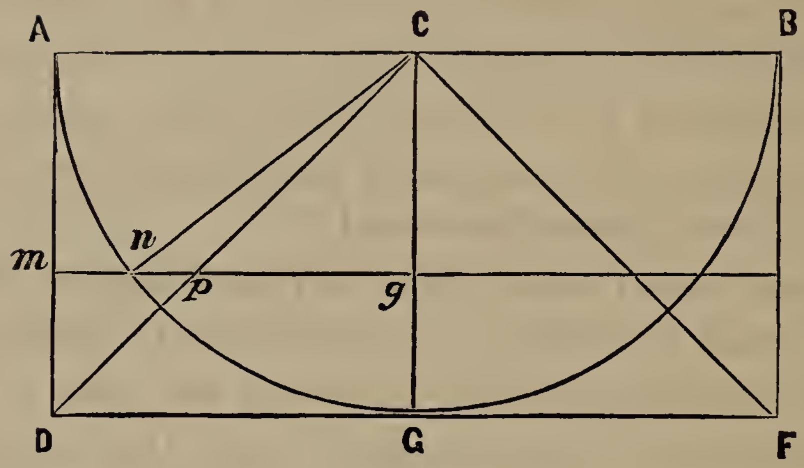
Many persons have embraced the axiom in question without seeming to know anything of the motives which induced a Pascal and a Roberval to invent and use it. Thus, for example, in a "Mathematical Dictionary and Cyclopedia of Mathematical Sciences," it is said, "When several quantities, either finite or infinitesimal, are connected together by the signs plus or minus, all except those of the lowest order may be neglected without affecting the value of the expression. Thus, $a + dx + dx^2 = a$."* Is it possible $a + dx + dx^2$ is exactly equal to a, and yet $dx + dx^2$ are really quantities? But, then, they are so very small that they may be added to a, without affecting its value in the least possible degree!

There is, it is true, high authority in favor of this

* Dictionary of Mathematics, etc., by Davies and Peck. Art. Infinitesimal.

strange axiom. Roberval, Pascal, Leibnitz, the Marquis de L'Hôpital, and others, have all lent the sanction of their great names to support this axiom and give it currency in the mathematical world. But does a real axiom ever need the support of authority? On the other hand, there is against this pretended axiom, as intrinsically and evidently false, the high authority of Berkeley, Maclaurin, Carnot, Euler, D'Alembert, Lagrange, and Newton, whose names preclude the mention of any other. But "where doctors disagree?" Doctors never disagree about the axioms of geometry. The very fact of a disagreement among them proves that it is not about an axiom, but only about something else which is set up as an axiom. It is, indeed, of the very essence of geometrical axioms that they are necessary and universal truths, absolutely commanding the assent of all, and shining, like stars, above all the dust and darkness of human controversy. But waiving this, I shall in the next chapter explode this pretended axiom, this principle of darkness assuming the form of light, which has so long cast its shadow on some of the fairest portions of demonstrative truth.

I shall conclude the present chapter with the examples which Carnot has given from Cavalieri and Pascal to illustrate and recommend the method of indivisibles. "Let A B," says he, "be the diameter of a semicircle, A G B: let A B F D be the circumscribed rectangle; C G the radius perpendicular to D F; let the two diagonals C D, C F also be drawn; and finally through any point m of the line A D, let the right line $m\ n\ p\ g$ be drawn perpendicular to C G, cutting the circumference of the circle at the point n, and the diagonal C D at the point p.



"Conceive the whole figure to turn around C G, as an axis; the quadrant of the circle A C G will generate the volume of a semi-sphere whose diameter is A B; the rectangle A D C G will generate the circumscribed right cylinder; the isosceles right-angled triangle C G D will generate a right cone, having the equal lines C G, D G for its height and for the radius of its base; and finally the three right lines or segments of a right line $m\,g$, $n\,g$, $p\,g$ will each generate a circle, of which the point g will be the centre.

"But the first of these circles is an element of the cylinder, the second is an element of the semi-sphere, and the third that of the cone.

"Moreover, since the areas of these circles are as the squares of their radii, and these three radii can evidently form the hypothenuse and the two sides of a right-angled triangle, it is clear that the first of these circles is equal to the sum of the other two; that is to say, that the element of the cylinder is equal to the sum of the corresponding elements of the semi-sphere and of the cone; and as it is the same with all the other elements, it follows that the total volume of the cylinder is equal to the sum of the total volume of the semi-sphere and of the total volume of the cone.

"But we know that the volume of the cone is one-third that of the cylinder; then that of the semi-sphere is two-thirds of it; then the volume of the entire sphere is two-thirds of the volume of the circumscribed cylinder, as Archimedes discovered."

Again, says Carnot, "the ordinary algebra teaches how to find the sum of a progression of terms taken in the series of natural numbers, the sum of their squares, that of their cubes, etc.; and this knowledge furnishes to the geometry of indivisibles the means of valuing the area of a great number of rectilinear and curvilinear figures, and the volumes of a great number of bodies.

Let there be a triangle, for example; let fall from its vertex upon its base a perpendicular, and divide this perpendicular into an infinity of equal parts, and lead through each of the points of division a right line parallel to the base, and which may be terminated by the two sides of the triangle.

According to the principles of the geometry of indivisibles, we can consider the area of the triangle as the sum of all the parallels which are regarded as its elements; but, by the property of the triangle, these right lines are proportioned to their distances from the vertex; then the height being supposed divided into equal parts, these parallels will increase in an arithmetical progression, of which the first term is zero.

But in every progression by differences of which the first term is zero, the sum of all the terms is equal to the last, multiplied by half the number of terms. But here the sum of the terms is represented by the area of the triangle, the last term by the base, and the number of terms by the height. Then the area of

every triangle is equal to the product of its base by the half of its height.

Let there be a pyramid; let fall a perpendicular from the vertex to the base; let us divide this perpendicular into an infinity of equal parts, and through each point of division pass a plane parallel to the base of this pyramid.

According to the principles of the geometry of indivisibles, the intersections of each of these planes by the volume of the pyramid will be one of the elements of this volume, and this latter will be only the sum of all these elements.

But by the properties of the pyramid these elements are to each other as the squares of their distances from the vertex. Calling, then, B the base of the pyramid, H its height, *b* one of the elements of which we have just spoken, *h* its distance from the vertex, and V the volume of the pyramid, we will have—

$$B : b :: H^2 : h^2;$$

therefore,

$$b = \frac{B}{H^2} h^2.$$

Then V, which is the sum of all these elements, is equal to the constant $\frac{B}{H^2}$ multiplied by the sum of the squares of h^2; and since the distances h increase in a progression by differences of which the first term is zero and the last H—that is, as the natural numbers from o to H—the quantities h^2 will represent their squares from o to H^2.

Now common algebra teaches us that the sum of

the squares of the natural numbers from o to H, inclusively, is

$$\frac{2H^3 + 3H^2 + H}{6}.$$

But here the number H being infinite all the terms which follow the first in the numerator disappear in comparison with this first term, then this sum of the squares is reduced to $\frac{1}{3}H^3$.

Multiplying, then, this value by the constant $\frac{B}{H^2}$, found above, we will have for the volume sought—

$$V = \tfrac{1}{3} B H;$$

that is, the volume of the pyramid is the third of the product of its base by its height."

Now here we see, in all their naked harshness, the assumption of Cavalieri on the one hand and that of Pascal on the other. An area is supposed to be made up of lines, of that which, compared with the unit of superficial measure, has absolutely no area at all! This hypothesis is, as we have seen, pronounced "certainly absurd" by Carnot, and yet it leads by some unknown process to true results. How this happened, or could have happened, Carnot is at no pains to explain. This seems the more extraordinary because the clue to the secret was more than once in his hands, and only required to be seized with a firm grasp and followed out to its consequences, in order to solve the enigma of "the method of indivisibles." He is, in fact, the apologist rather than the expounder of that method.

No one was more sensible than Cavalieri himself

of the grave objections to his own method. Accordingly he strove, as he tells us, "to avoid the supposing of magnitude to consist of indivisible parts," because there remained some difficulties in the matter which he was not able to resolve.* Instead of pretending that he could explain, or even see through, these objections, he exclaimed: "Here are difficulties which the arms of Achilles could not conquer." He speaks, indeed, as if he foresaw that his method would be, at some future day, delivered in an unexceptionable form, so as to satisfy the most scrupulous geometrician. But free from the miserable sham of pretending to understand it himself, he simply leaves, with a beautiful candor worthy of his genius, this *Gordian knot*, as he calls it, to some future *Alexander*. If that *Alexander* appeared in the person of Carnot, it must be admitted that, like the original, he was content to cut rather than to untie the *Gordian knot* of the method of indivisibles.†

Again, we are gravely told that infinity, plus 3 times infinity square, may be neglected, or thrown out as nothing, by the side of infinity cube. Now such propositions (I speak from experimental knowledge) tend to disgust some of the best students of science with the teachings of the calculus, and to inspire nearly all with the conviction that it is merely a method of approximation. How could it be otherwise? How can reflecting minds, or such as have been trained and encouraged to *think*, be told, as we are habitually told in the study of the differential calculus, that certain

* Cavalieri, Geom. Indivis., lib. 7.

† I speak in this way, because in my laborious search after light respecting the enigma of the method of Cavalieri, I applied to Carnot in vain.

quantities are thrown out or neglected on one side of a perfect equation, without feeling that its perfection has been impaired, and that the result will, therefore, be only an approximation to the truth? This is the conclusion of nearly all students of the calculus, until they are better informed by their instructors. Every teacher of the calculus is often called upon to encounter this difficulty; but, unfortunately, few are prepared to solve it either to their own satisfaction or to that of their pupils.

Thus, for example, in one of the latest and best treatises on the "Differential Calculus" which has been issued from the University of Cambridge, we find these words: "A difficulty of a more serious kind, which is connected with the notion of a limit, appears to embarrass many students of this subject—namely, a suspicion that the methods employed are only approximative, and therefore a doubt as to whether the results are absolutely true. This objection is certainly very natural, but at the same time by no means easy to meet, on account of the inability of the reader to point out any definite place at which his uncertainty commences. In such a case all he can do is to fix his attention very carefully on some part of the subject, as the theory of expansions for example, where specific important formulæ are obtained. He must examine the demonstrations, and if he can find no flaw in them, he must allow that results *absolutely true and free from all approximation* can be legitimately derived by the doctrine of limits." *

Alas! that such teaching should, in the year of

* Todhunter's Differential Calculus, etc. Cambridge: Macmillan & Co. 1855.

grace 1866, issue from the most learned mathematical University in the world, and that, too, nearly two centuries after its greatest intellect, Newton, had created the calculus! What! the reader, the student not able to point out the place at which his difficulty begins! Does not every student know perfectly well, in fact, that when he sees small quantities neglected, or thrown out on one side of an equation, and nothing done with them on the other, he then and there begins to suspect that the calculus is merely an approximative method? In view of the rejection of such quantities his "objection is," as the author says, "certainly very natural." Nay, his "suspicion" is not only natural; it is necessary and inevitable. But if any student should be unable to tell where his "difficulty," his "suspicion," his "uncertainty" commences, why should not this be pointed out to him by his teacher? Surely, after the labors of a Berkeley, a Carnot, a D'Alembert, and of a hundred more, the teacher of mathematics in the most learned University in the world should be at no loss either to explain the origin of such a difficulty, or to give a rational solution of it. Is the philosophy, the theory, the rationale of the infinitesimal calculus not at all studied at Cambridge? The truth is, that the teacher in question, like many others, found it "by no means easy to meet" *the* difficulty which haunts the mind of every student of the calculus, just because he himself had studied the wonderful creation of Newton merely as a practical art to be used, and not as a glorious science to be understood.

CHAPTER IV.

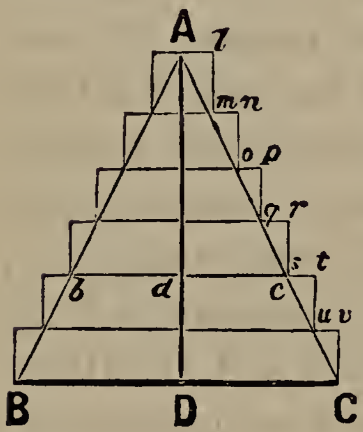
SOLUTION OF THE MYSTERY OF CAVALIERI'S METHOD, AND THE TRUE METHOD SUBSTITUTED IN ITS PLACE.

In the preceding chapter, the difficulty, the enigma, the mystery of Cavalieri's method was fully exhibited. It is my object, in this chapter, to clear up the mystery of that method, and to set the truth in a transparent and convincing point of view. Or to untie, as he calls it, "the Gordian knot" of his method, and to replace it by a perfectly clear train of reasoning, which shows the necessary connection between undenied and undeniable principles, and the conclusion at which he arrived, as well as conclusions lying beyond the reach of his obscure and imperfectly developed system.

I shall begin with the first of the examples or illustrations produced from the work of Carnot. By consulting the last chapter the reader will perceive that Cavalieri finds the area of any triangle by obtaining, as he supposes, the sum of its elements or of all right lines parallel with its base, and included between its two sides. Now, although this hypothesis is "certainly absurd," yet is there at the bottom of it a profound truth which was most obscurely seen, and therefore most inadequately expressed, by the great Italian. Nor from that day to this has the truth in question been any better seen or more adequately expressed — a

fact which will in due time be demonstrated in the following pages. As often as the mathematician has by his reasoning been brought face to face with this great truth he has failed to see it, because he has misconceived and misinterpreted his symbols. But we are not, as yet, quite prepared to set this singular and instructive fact in a perfectly satisfactory and convincing light.

Let us, then, return to the case of the triangle, which is represented by the figure A B C. Let its altitude A D be divided into any number of equal parts, as seen in the figure, and through each point of division let a right line be drawn parallel to its base and terminating in its two sides. Let there be, as in the figure, a system of rectangles constructed, each having in succession one of the parallel lines for its base. Now the question is, what course should the geometer pursue in order to obtain by a clear and unexceptionable logical process the area of the triangle A B C?



Cavalieri, as we have already seen, would not proceed on the assumption that the sum of the rectangles, how great soever their number, is equal to the area of the triangle, because he believed that it would always be greater than that area. Hence, in order to arrive at the exact area, he conceived the triangle to be composed, not of rectangles however small, but of right lines. Pascal, on the other hand, acknowledging the absurdity of such an hypothesis, supposed the triangle to be composed of the rectangles when their number

was indefinitely increased. Thus, by a slight divergence between the courses of the two great geometers, the one was landed in Scylla and the other in Charybdis.

The method of Pascal is founded in error. Its basis, its fundamental conception, is demonstrably false. It is evident that the sum of the rectangles can never be exactly equal to the area of the triangle unless the broken line A $l\ m\ n\ o\ p\ q\ r\ s\ t\ u\ v$ C can be made to coincide with the line A C. But this can never be, since, however great the number of rectangles may be conceived to be, still the sum of all the little lines, such as A l, $m\ n$, $o\ p$, and so forth, parallel to the base of the triangle will always continue equal to D C, and the sum of all the little lines, such as $n\ o$, $p\ q$, $r\ s$, and so forth, parallel to the altitude A D of the triangle, will always continue equal to A D. Hence the broken line A $l\ m\ n\ o\ p\ q\ r\ s\ t\ u\ v$ C will always remain equal to D C + A D; and if it should ever coincide with A C, then one side of the triangle A D C would be equal to the sum of the other two, or the hypothenuse of a right-angled triangle would be equal to the sum of its two sides, which is impossible. Indeed, the broken line in question is a constant quantity; the number of parallels may be increased *ad libitum*, and yet the length of the broken line will remain invariably the same. Hence the difference between this constant length and the length of A C is itself a constant quantity, and the length of the one line never even approximates to that of the other, much less can the one ever coincide with the other. A C is not even the limit of the broken line A $l\ m\ n$, etc., since the value of the latter does not tend toward that of the former as the

number of its parts is increased. But the area of the triangle A D C is the limit of the area of the figure C D A $l\,m\,n\,o\,p\,r\,s\,t\,u\,v$ C, since the last area continually tends toward an equality with the first area, without ever becoming absolutely equal to it. The same things are, it is obvious, equally true in regard to the right line A B, and the broken line on the other side of the triangle A B C.

We should, then, discard the fundamental conception of Pascal and Roberval as false; which we may do at the present day without falling into the hypothesis of Cavalieri or any of its manifold obscurities. If, instead of seeking the sum of the rectangles, whose number is supposed to be indefinitely increased, we seek the *limit* of that sum, we shall find the exact area of the triangle by a logical process as clear in itself as it is true in its conclusion.

For this purpose let B represent the base of the triangle A B C, b the base $b\,c$ of any triangle, A $b\,c$ formed by one of the lines parallel to B C, H and h the respective heights of these two triangles, and k one of the equal parts into which the line A D has been divided. Then, by similar triangles, we have

$$b : \mathrm{B} :: h : \mathrm{H},$$

or

$$b = \frac{\mathrm{B}}{\mathrm{H}} \cdot h,$$

$$b\,k = \mathrm{k}\,\frac{\mathrm{B}}{\mathrm{H}} \cdot h,$$

in which $b\,k$ is the area of the little rectangle, whose base is b and altitude k. Now, the limit of the sum of all such rectangles being the exact area of the triangle

A B C, we have only to find the limit of that sum in order to obtain an expression for the area sought. That is to say, we have only to find the limit of the sum of $k\frac{B}{H}.h$ for all the values of h. But the value of h varies from A to A D or from zero to H, and since the heights of the little rectangles are all equal to each other, we shall have for the successive values of h,

$$k,\ 2k,\ 3k\ldots nk,$$

in which n denotes the whole number of rectangles, or of equal parts into which A D is divided. Let it be observed that

$$nk = AD = H.$$

Then the sum of

$$k\frac{B}{H}h = k\frac{B}{H}(k + 2k + 3k\ldots + nk),$$

or
$$k\frac{B}{H}h = k^2\frac{B}{H}(1 + 2 + 3\ldots + n).$$

But since the sum of the series $1 + 2 + 3\ldots + n$ is, according to a well-known algebraic formula, equal to $\frac{n(n+1)}{2}$, we have

$$k^2\frac{B}{H}(1 + 2 + 3\ldots n) = k^2\frac{B}{H} \times \frac{n(n+1)}{2}$$

$$= \frac{B}{H}.\frac{nk(nk+k)}{2} = \frac{B}{H} \times \frac{H(H+k)}{2} = \frac{B}{H}$$

$$\times \frac{H^2 + Hk}{2}.$$

Now, if S be the sum of the rectangles, we shall have

$$S = \frac{B}{H} \times \frac{H^2 + H k}{2}.$$

However small k may be made, or however great, in other words, the number of rectangles may be conceived to be, the two variables S and its value will be equal to each other. Hence, as has been demonstrated, their limits are equal. But the limit of S is the area of the triangle A B C, and the limit of $\frac{B}{H} \times \frac{H^2 + H k}{2}$ is $\frac{B}{H} \times \frac{H^2}{2}$, or $\frac{B H}{2}$. That is, the area of the triangle A B C is one-half the product of its base by its altitude.

Now, it may be clearly shown how it was that Pascal, as well as Roberval and others, started from a false hypothesis or first principle, and yet arrived at a perfectly correct conclusion. He committed an error first in supposing that the sum of the rectangles would ultimately be equal to the area of the triangle; he committed another error, in the second place, in supposing that he could reject indefinitely small quantities without making any difference in the result; and these two errors, being opposite and equal, just exactly neutralized each other. Thus, the quantities which he rejected did make a most important difference in the result, for they made it exactly true instead of false. It is, in the natural world, experimentally proved that two rays of light may cross each other so as to produce darkness. But this is nothing to the wonder of the infinitesimal method as used by Rober-

val and Pascal. For here two rays of darkness are made to produce light.

Thus, in the logic of Pascal, there was an unsuspected compensation of unsuspected errors. This might, indeed, have been conjectured from the nature of his procedure. For, if we look at the figure, we shall perceive that the sum of the rectangles is made up of the triangle A B C, which is always constant, and of all the little variable triangles which serve to complete that sum. In like manner, if we examine the expression for the sum of the rectangles, we shall find that it is composed of a constant term and of a variable term. For that expression is, as we have seen, $\frac{B}{H} \times \frac{H^2 + H}{2}$, or $\frac{BH}{2} + \frac{B}{2}$, an expression which, literally understood, has no meaning. For $\frac{BH}{2}$ is a surface, and $\frac{B}{2}$ is a line, and it is impossible to add a line to a surface. Hence, according to the well-known principle of homogeneity, we must in all such cases restore the understood unit of measure, which is, in the present case, the variable quantity k. The above expression then becomes $\frac{BH}{2} + \frac{Bk}{2}$. The constant term $\frac{BH}{2}$ is the measure of the constant triangle A B C. Is not the variable term, then, $\frac{Bk}{2}$, the expression for the sum of all the little variable triangles? That is to say, have not all these little triangles been added to the area A B C, and then thrown away as if they were nothing in their last stage of littleness? Such a

suspicion, it seems to me, ought to have arisen in the mind of any one who had looked closely and narrowly into the mysteries of this method.

But this charge of a compensation of errors is something more than a shrewd suspicion or conjecture. It is a demonstrative certainty. The opposite errors may be easily seen and computed, so as to show that they exactly neutralize each other. Thus, when it is asserted that the triangle A B C is equal to the sum of all the rectangles set forth in the figure, it is clear that the measure is too great, and exceeds the area of A B C by the sum of all the aforesaid little triangles. But the rejected term $\frac{\mathrm{B}\,k}{2}$ is exactly equal to that excess, or to the sum of all the little triangles. For the triangle $\mathrm{A}\,l\,m = \frac{\mathrm{A}\,l \times k}{2}$, $m\,n\,o = \frac{m\,n \times k}{2}$, $o\,p\,q = \frac{o\,p \times k}{2}$, $q\,r\,s = \frac{q\,r \times k}{2}$, $s\,t\,u = \frac{s\,t \times k}{2}$, and $u\,v\,\mathrm{C} = \frac{u\,v \times k}{2}$. Hence their sum is equal to

$$\frac{(\mathrm{A}\,l + m\,n + o\,p + q\,r + s\,t + u\,v)\,k}{2} = \frac{\mathrm{D\,C} \times k}{2}.$$

In like manner it may be shown that the sum of the triangles on the other side of the triangle A B C is equal $\frac{\mathrm{B\,D} \times k}{2}$. Hence the sum of all the triangles on both sides of A B C is equal to $\frac{(\mathrm{D\,C} + \mathrm{B\,D})\,k}{2} = \frac{\mathrm{B}\,k}{2}$. But this is precisely the quantity which has been thrown away, as so very small as to make absolutely no difference in the result! It is first added

by a false hypothesis, and then rejected by virtue of a false axiom, and the exact truth is reached, both to the astonishment of the logician—not to say magician—and of all the world beside.

If we may, openly and above-board, indulge in such a compensation of errors, then we need not go down into the darkness of the infinite at all. For the above reasoning—if reasoning it may be called—is just as applicable to a finite as it is to an infinite number of terms. Let us suppose, for example, that the number of rectangles constructed, as above, are finite and fixed instead of variable and indefinite. Let this finite fixed number be denoted by n and the sum of the rectangles by S.

Then $$S = \frac{BH}{2} + \frac{Bk}{2}.$$

Now, if we may be permitted to assert, in the first place, that this sum is equal to the area of the triangle, and, in the second, throw away $\frac{Bk}{2}$ as unworthy of notice, then we shall obtain $\frac{BH}{2}$, or one-half the product of the base by the height as an expression for the area of the triangle. The result is exactly correct. But, then, in asserting that the sum of the rectangles, say of ten for example, is equal to the triangle, we make its area too great by the sum of twenty very respectable triangles. We correct this error, however, by throwing away $\frac{Bk}{2}$, or rather $\frac{B \cdot \frac{1}{10} H}{2}$, which is exactly equal to the sum of these twenty triangles.

Precisely such, in nature and in kind, is the reasoning of the more approved form of the method of indivisibles. It is, indeed, only under the darkness of the infinite that such assertions may be made and such illicit processes carried on without being detected, and they expire under the scrutiny of a microscopic inspection.

How different the method of limits! If properly understood, this proceeds on no false assertion and perpetrates no illicit process. No magnified view can be given to this method which will show its proportions to be otherwise than just or its reasonings to be otherwise than perfect. Having found the above expression for the sum of its auxiliary rectangles, which is $S = \frac{B}{H} \times \frac{H^2 + H}{2}$, this method does not throw away H in the numerator of the last term, because H, though infinite, may therefore be treated as nothing by the side of H^2. On the contrary, it simply makes that term homogeneous by restoring the suppressed or understood unit of measure k, so that it becomes

$$S = \frac{B\,H}{2} + \frac{H\,k}{2};$$

and then proceeds on the demonstrated truth that if two variables are always equal, their limits must also be equal. But the area of the triangle is the limit of S (the sum of the rectangles), and $\frac{B\,H}{2}$ is the limit of the second member of the above equation. Hence, if A be the area of the triangle, we have $A = \frac{B\,H}{2}$.

In like manner, from the expression $\dfrac{2\,H^3 + 3\,H^2 + H}{6}$ found by Carnot in the last chapter, the method of limits does not reject infinity, plus 3 times infinity square, as nothing by the side of twice infinity cube, in order to reach the conclusion that the whole expression is exactly equal to the needed result $\frac{1}{3}\,H^3$. In the sublime philosophy of Pascal, "the number H being infinite, all the terms which follow $+ 2\,H^3$ in the numerator disappear by the side of that first term; then that sum of the squares reduces itself to $\frac{1}{3}\,H^3$." But the method of limits, more humble and cautious in its spirit, takes its departure from the demonstrated proposition that if two variable quantities are always equal, then their limits must be equal, and arrives at precisely the same result. For $\dfrac{2\,H^3 + 3\,H^2 + H}{6}$, when fully expressed, is $\dfrac{2\,H^3}{6} + \dfrac{3\,H^2\,k}{6} + \dfrac{H\,k^2}{6}$, and by making $k = o$, we find its limit $\frac{1}{3}\,H^3$.

From the above example it will be seen that Pascal, instead of taking the sum of his auxiliary rectangles and the sum of his auxiliary prisms, as he supposed he did, in finding the area of a triangle and the volume of a pyramid, really took the *limits* of those sums, and that, too, without even having had the idea of a limit, or comprehending the nature of the process he performed. Nor is this all. For he arrived at this result only by a one-sided and partial application of his own principle. In order to explain, let us resume the above expression for the sum of the auxiliary rectangles, which is $\dfrac{B}{H} \times \dfrac{H^2 + H}{2}$. Now if H be

infinite, and may be omitted as nothing compared with H^2, reducing the last factor to $\frac{H^2}{2}$, it should be remembered that, according to the same supposition, the first factor becomes $\frac{B}{\infty} = 0$. Hence the expression for the sum of the rectangles is reduced to $0 \times \infty$, or an infinite number of zeros. Precisely the symbol of the great truth which lies at the bottom of Cavalieri's hypothesis, and which, as we shall hereafter see, still remains to be correctly interpreted by the mathematical world. In like manner the sum of the auxiliary prisms used in finding the volume of the pyramid, or $S = \frac{B}{H^2} \times \frac{2H^3 + 3H^2 + H}{6}$, is reduced by the same suppositions to $S = \frac{B}{\infty} \times \frac{\infty}{6} = 0 \times \infty$, a symbol which never could have been understood or correctly interpreted without a knowledge of the method of limits. But ever since that knowledge has been possessed and more clearly developed, the meaning of the symbol $0 \times \infty$ has been, as it were, looking the mathematician in the face and waiting to be discovered. No attempt can, however, be made to construe it, until the methods of Leibnitz and Newton be passed under review.

Before leaving this branch of the subject, it may be well to show how, by the method of limits, the volume of the pyramid is determined. Let V, then, be the volume of any pyramid, B its base, and H the perpendicular from its vertex on the plane of its base. Let H be divided into any number of equal parts, each represented by k, and planes passed through the several points of division parallel to the base. On the base

of the pyramid, and on every similar section of the pyramid cut out by the parallel planes, conceive right prisms to be constructed, each equal in height to k, the distance between any two adjacent parallel planes. Let S represent the sum of these prisms, b the base of any one of them except the lowest, and h its distance from the vertex of the pyramid.

Then, by a well-known property of the pyramid, we shall have

$$b : \mathrm{B} :: h^2 : \mathrm{H}^2$$

or
$$b = \frac{\mathrm{B}}{\mathrm{H}^2} \,.\, h^2,$$

and
$$b \,.\, k = \frac{\mathrm{B}\,k}{\mathrm{H}^2} \,.\, h^2,$$

for the volume of the prism whose base is b. Now S, the sum of all the prisms, is evidently equal to $\frac{\mathrm{B}\,k}{\mathrm{H}^2}$ multiplied into the several values of h^2. But, if n be the whole number of prisms, then the several values of h will be

$$k,\ 2\,k,\ 3\,k,\ 4\,k \ldots + n\,k. \qquad n\,k = \mathrm{H}.$$

Hence,
$$\mathrm{S} = \frac{\mathrm{B}\,k}{\mathrm{H}^2}\,(k^2 + 2^2\,k^2 + 3^2\,k^2 + 4^2\,k^2 \ldots n^2\,k^2),$$

or
$$\mathrm{S} = \frac{\mathrm{B}\,k^3}{\mathrm{H}^2}\,(1 + 2^2 + 3^2 + 4^2 \ldots + n^2).$$

But, according to a well-known algebraic formula,

$$(1 + 2^2 + 3^2 + 4^2 \ldots + n^2) = \frac{n\,(n+1)\,(2\,n+1)}{1 \,.\, 2 \,.\, 3}.$$

Hence, $S = \frac{B}{H^2} \cdot \frac{nk(nk + k)(2nk + k)}{1.2.3}$,

or $$S = \frac{B}{H^2} \cdot \frac{H(H + k)(2H + k)}{1.2.3}$$

$$= \frac{B}{H^2} \times \frac{2H^3 + 3H^2k + Hk^2}{1.2.3}.^*$$

Now, if we conceive k to become smaller and smaller, or the number of prisms to become greater and greater, their sum will continually tend more and more to an equality with the volume of the pyramid, without ever becoming exactly equal to that volume. Hence V is the limit of S. In like manner, as k becomes smaller and smaller, the expression

$$\frac{B}{H^2} \times \left(\frac{2H^3 + 3H^2k + Hk^2}{1.2.3}\right),$$

or $$\frac{B}{H^2} \times (\tfrac{1}{3}H^3 + \tfrac{1}{2}H^2k + \tfrac{1}{6}Hk^2),$$

tends continually more and more toward an equality with $\frac{B}{H^2} \times \frac{1}{3}H^3$, without ever reaching that value, while k remains a real quantity, or the prisms have the least possible thickness. Hence $\frac{B}{H^2} \times \frac{1}{3}H^3$ is the limit of the variable in question.

But as these two variables are always equal, then are their limits also equal. That is to say,

* Here, as the unit of measure k is not dropped or suppressed; the expression is homogeneous, as it should always be understood to be even when not expressed.

$$\text{limit of } S = \text{limit of } \frac{B}{H^2} \times (\tfrac{1}{3} H^3 + \tfrac{1}{2} H^2 k + \tfrac{1}{6} H k^2),$$

or $$V = \frac{B}{H^2} \times \tfrac{1}{3} H^3 = \tfrac{1}{3} B H;$$

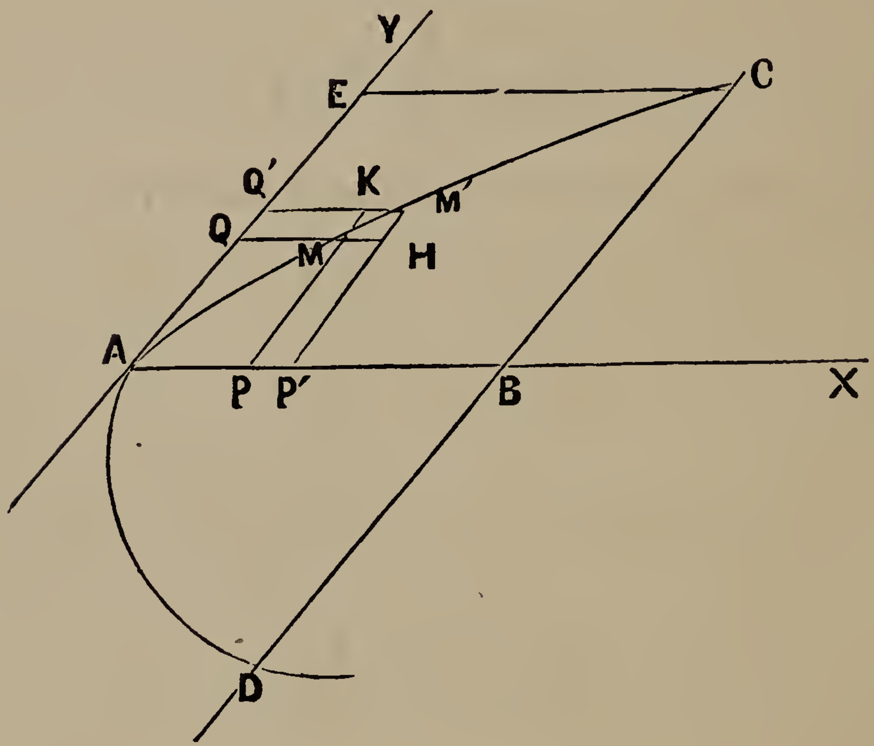
the well-known measure for the volume of a pyramid.

In the above example I have used a good many words, because the beginner, for whom it is written, is not supposed to be familiar with the method of limits. But the process is in itself so direct, simple, and luminous, that a little familiarity with the method of limits will enable the student to repeat it or any similar process almost at a glance. He will only have to conceive the pyramid with its system of auxiliary prisms, form the expression for their sum, pass to its limits, and the problem is solved, or the volume of the pyramid found. And he may do this, too, with little or no aid from the use of diagrams or symbols. He may, in fact, bring his mind into direct contact with geometrical phenomena, and reason out his results in full view of the nature of things, or of their relations, rather than in the blind handling of mere formulæ, and thus beget a habit of meditation and of close discriminating attention, which are among the very best effects of any system of mental education.

AREA OF THE PARABOLIC SEGMENT.

This question will offer us examples of very various procedures which may be employed in the search of quadratures, and will give an idea of the variety of resources which the infinitesimal method presents.

First Solution.— Referring the parabola to the diameter A B and to the tangent A Y parallel to the base of the segment A D C, the squares of the ordinates



are proportional to the abscissas, and the equation of the curve is

$$y^2 = 2 p x.$$

Let us cut the surface by parallels to A Y; the area A D C may be considered as the limit of the sum of interior parallelograms, which will be divided into two equal parts by the diameter A B, so that the two areas A B C, A B D, being limits of equal sums, are equivalent. It suffices, then, to calculate the half A B C.

We shall know the area A B C if we know its ratio to the complement A E C of this area in the parallelogram A B C E, and to find this we will compare two corresponding parallelograms P M H P′, Q M K Q′ of the sums which have these two areas for their limits.

Designating by x, y the co-ordinates of any point M, and by h and k the increments that they acquire in passing from M to M′, we will have

$$\frac{\text{P M H P}'}{\text{Q M K Q}'} = \frac{y\,h}{x\,k},$$

and we have, from the equation of the curve,

$$y^2 = 2\,p\,x; \ (y + k)^2 = 2\,p\,(x + h);$$

hence $$2\,y\,k + k^2 = 2\,p\,h.$$

Hence,

$$\frac{h}{k} = \frac{2\,y + k}{2\,p}; \text{ or } \frac{y}{x} \times \frac{h}{k} = \frac{y}{x} \times \frac{2\,y + k}{2\,p} = \frac{2\,y^2 + k\,y}{2\,p\,x};$$

or $$\frac{y\,h}{x\,k} = 2 + \frac{k\,y}{2\,p\,x}.$$

This ratio then tends toward the limit 2, when k tends toward zero; the two areas A C B, A E C are then the limits of sums of such infinitely small quantities that the ratio of any two corresponding ones tends toward the same limit 2; then, according to the principle already demonstrated, the ratio of the areas A C B, A E C is exactly 2. Thus, the area of A C B is two-thirds of the parallelogram A E C B, and the proposed area A C D is four-thirds of this same parallelogram, or two-thirds of the whole circumscribed parallelogram.

Second Solution.—It is easy to calculate directly the area A E C, which is the limit of the sum of the parallelograms Q M K Q′ or Q H M′ Q′, of which the general expression is $x\,k$ sin A, A designating the

angle Y A X, k designating the increment of y. It is necessary to express x in terms of y, which will give for the expression of any one of the parallelograms $\frac{y^2 k \sin A}{2p}$. Now, if we suppose in this case that the altitudes of the parallelograms are all equal, which was useless in the preceding solution, the successive values of y will be

$$k,\ 2k,\ 3k \ldots nk,$$

and we shall have

$$nk = A\,E, \text{ or } (n+1)k = A\,E,$$

according as we take the parallelograms Q M′ or Q K, which is indifferent.

It is required, then, to find the limit of the sum

$$\frac{k^3 \sin A}{2p}(1 + 2^2 + 3^2 + \ldots + n^2),$$

when n increases indefinitely, and k decreases at the same time, so that we always have $nk = A\,E$.

Now, Archimedes has given for the summation of the squares of the natural numbers a formula which, written with the signs used by *the moderns*, gives

$$1 + 2^2 + 3^2 + \ldots n^2 = \frac{n(n+1)(2n+1)}{1 \,.\, 2 \,.\, 3}.$$

It is necessary, then, to find the limit of the following expression—

$$\frac{k^3 \sin A}{2p} \times \frac{n(n+1)(2n+1)}{1 \,.\, 2 \,.\, 3},$$

or $$\frac{A\,E(A\,E + k)(2\,A\,E + k) \sin A}{2 \,.\, 2 \,.\, 3p},$$

when k tends towards zero. That limit is evidently

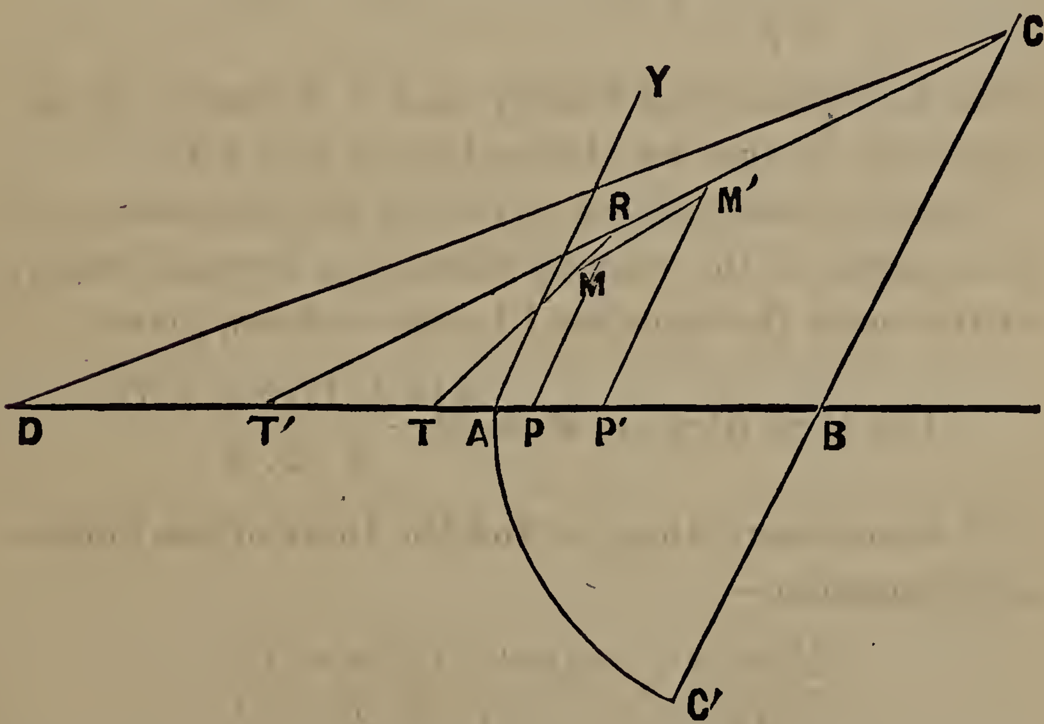
$$\frac{\overline{A\,E}^3 \,.\, \sin A}{2\,.\,3\,p}, \text{ or } \frac{A\,B : A\,E \sin A}{3},$$

observing that $\dfrac{\overline{A\,E}^2}{2\,p} = A\,B.$

The area A E C is, then, the third of the parallelogram A B C E, and the area A B C is two-thirds of it, as we found by the first solution.

Third Solution.—This solution will have the advantage of giving an example of a mode of *decomposition* very different from the preceding ones. We shall in this consider an area as the limit of a sum of areas, indefinitely small, determined by tangents to the same curve.

Let A C C′ be the parabolic segment, A B the



diameter, C D the tangent at C; from which results A D = A B; let us compare the two areas A C B, D A C.

We may consider A C B as the limit of a sum of inscribed trapeziums P M M′ P′, whose sides P P′ lying upon A B tend all in any way whatever towards zero.

As to the area D A C, we will draw at M and M′ the two tangents M T, M′ T′, from which will result A T = A P, A T′ = A P′, T T′ = P P′. If, through the point of meeting R of these tangents, we draw a parallel to A B, we shall have the diameter of the chords M M′, which will pass in consequence through the middle of M M′, so that the area of the triangle T R T′ will be half of that of the trapezium P P′ M M′. Now it is easy to see that the area of D A C is the limit of the sum of the triangles T′ T R. In fact, this area is exactly the sum of the areas comprised between each of the arcs M M′, the tangent M′ T′, the base of T′ T and the tangent T M terminating in M. But each of these areas differs from the corresponding triangle T′ T R by a quantity infinitely small in comparison with it, when P P′ tends towards zero; for this difference is less than the rectilinear triangle M R M′ whose ratio to the triangle T′ R T is that of the rectangles of the sides which include their angles at R, which are supplementary; a ratio which is evidently infinitely small. Then the area D A C is the limit of the sum of the triangles T′ R T.

This being established, the two areas A C B, D A C being limits of sums of infinitely small quantities which are in the ratio of 2 : 1, will be themselves in this ratio. Then A C B is two-thirds of the triangle D B C, or of the parallelogram constructed upon A B and B C, which leads us back to the result obtained before.

CHAPTER V.

THE METHOD OF DESCARTES, OR ANALYTICAL GEOMETRY.

Descartes is the great connecting link between the ancient and the modern geometry. For two thousand years, or a little less, the science of geometry had remained nearly stationary when this extraordinary man appeared to give it a new and prodigious impulse. During that long and dreary period not one original mind dared to assert its own existence. "It is not surprising," says the Marquis de L'Hôpital, "that the ancients did not go farther; but we know not how to be sufficiently astonished that the great men—without doubt as great men as the ancients—should so long have stopped there, and that, by an admiration almost superstitious for their works, they should have been content to read them and to comment upon them without allowing themselves any other use of their lights than such as was necessary to follow them, without daring to commit the crime of sometimes thinking for themselves, and of carrying their mind beyond what the ancients had discovered. In this manner many worked, wrote, and books multiplied, but yet nothing advanced; all the productions of many centuries only sufficed to fill the world with respectable commentaries and repeated translations of originals often sufficiently contemptible." Thus, there

was, in the mathematical world, no little activity; but it moved on hinges, not on wheels. It repeated, for the most part, the same everlasting gyrations, but made no progress.

"Such was the state of mathematics," continues the Marquis, "and above all philosophy, up to the time of *M. Descartes*. That great man, impelled by his genius, and by the superiority which he felt in himself, quitted the ancients to follow the same reason which the ancients had followed; and that happy boldness in him, though treated as a revolt, was crowned with an infinity of new and useful views concerning Physics and Geometry."

The Marquis knew, of course, that there were some exceptions to the above general statement. The time was sufficiently gloomy, it must be conceded, both with respect to mathematics and philosophy; but it was, nevertheless, relieved by the auspicious dawn that ushered in the brilliant era of *Descartes*. Algebra had been created, and *Vieta*, himself a man of great original genius, had effected that happy alliance between algebra and geometry which has been the prolific source of so many important results. But this detracts nothing from the glory of *Descartes*. For it is still true of him, as de L'Hôpital says, that "he commenced where the ancients had finished, and began by a solution of the problem at which *Pappus* said they had all been arrested. Nor is this all. It is merely the first step in his great career. He not only solved the problem which had, according to *Pappus*, proved too much for all the ancients, but he also invented a method which constitutes the foundation of the modern analysis, and which renders the most diffi-

cult questions considered by the ancients quite too easy and simple to tax even the powers of the merest tyro of the present day. The method which he discovered for tangents, the one great and all-comprehending question of the modern analysis, appeared to him so beautiful that he did not hesitate to say, "*That that problem was the most useful and the most general not only that he knew, but even that he ever desired to know in geometry.*" *

But although *Descartes*, like every true king of thinkers, extended the boundaries of science, he could not set limits to them. Hence, it was only a little while after the publication of his method for tangents, that *Fermat* invented one which *Descartes* himself admitted to be more simple and felicitous than his own.† It was the invention or discovery of his method of tangents which led Lagrange, in opposition to the common opinion, to regard *Fermat* as the first author of the differential calculus. But the method of Barrow was more direct and simple, if not more accurate, than that of *Fermat*. He assumed that a curve is made up of an infinite number of infinitely small right lines, or, in other words, to be a polygon, the prolongation of whose infinitely small side is the tangent to the curve at the point of contact. On this supposition the "differential triangle" formed by the infinitely small side of the polygon, the difference between the two ordinates to the extremities of that side, and the difference between the two corresponding abcissas, is evidently similar to the triangle formed by the tangent, the ordinate, and the subtangent to the point of contact. Hence the subtangent is found simply by means

* Geometrie, Liv. 2.

† Lettre 71, Tom. 3.

of these two similar triangles, a method which dispenses with the calculations demanded by the method of *Fermat*, as well as by that of *Descartes*.

Barrow did not stop, however, at his "*differential triangle;*" he invented a species of calculus for his method. But it was necessary for him, as well as for *Descartes*, to cause fractions and all radical signs to disappear in order to apply or use his calculus. This was, says the Marquis de L'Hôpital, "the defect of that calculus which has brought in that of the celebrated *M. Leibnitz*, and that learned geometer has commenced where M. Barrow and the others had terminated. His calculus has led into regions hitherto unknown, and made those discoveries which are the astonishment of the most skillful mathematicians of Europe. The Messrs. *Bernouilli* (and the Marquis might have added himself) were the first to perceive the beauty of that calculus; they have carried it to a point which has put it in a condition to surmount difficulties which no one had ever previously dared to attempt.

"The extent of that calculus is immense; it applies to mechanical curves as well as to geometrical; radical signs are indifferent to it, and even frequently convenient; it extends to as many indeterminates as one pleases; the comparison of infinitely small quantities of all kinds is equally easy. And hence arises an infinity of surprising discoveries with respect to tangents, whether curvilinear or rectilinear ones, to questions of *maxima* and *minima*, to points of inflexion and of rebrousement of curves, to developées, to caustics by reflexion and by refraction," etc.*

Thus, by his method of tangents, *Descartes* opened

* Preface to Analyse des Infinités Petites.

the direct route to the differential calculus. Nor is this all. For, by the creation of his co-ordinate geometry, he enabled Fermat, Barrow, Newton, and Leibnitz to travel that route with success. A more happy or a more fruitful conception had never, up to that time, emanated from the mind of man, than *Descartes'* application of indeterminate analysis to the method of co-ordinate geometry—a method which was due exclusively to his own genius.

We shall, then, proceed to give, as far as possible, an accurate and complete idea of Aanalytical Geometry—the wonderful method created by Descartes. This branch of mathematics has one thing in common with the application of algebra to geometry—namely, the use of algebraic symbols and processes in the treating of geometrical questions. Hence, if we would obtain clear views respecting its first principles or its philosophy, we must, in the first place, form a just idea of the precise relation which these symbols sustain to geometrical magnitudes. We proceed, then, to illustrate and define this relation.

THE RELATION OF ALGEBRAIC SYMBOLS TO GEOMETRICAL MAGNITUDES.

On this subject geometers have advanced at least three distinct opinions. The first is, that in order to represent the length of a line (to begin with the most simple case) by a letter, we must apply to it some assumed unit of lineal measure, as a foot or a yard, and see the number of times it contains this unit. Then this number may, as in ordinary algebra, be represented by a letter. According to this view, the number represents the line and the letter the number.

Such process of comparison, it is supposed, must either be executed or conceived in order to establish the *possibility* of expressing geometrical magnitudes by the characters of algebra.

The second opinion is, that "geometrical magnitudes may be represented algebraically in two ways: first, the magnitudes may be directly represented by letters, as the line A B, given absolutely, may be represented by the symbol a; secondly, instead of representing the magnitudes *directly*, the algebraic symbols may represent the number of times that a given or assumed unit of measure is contained in the magnitudes; as for the line A B, a may represent the number of times that a known unit is contained in it."

A a B

In this case, as it is said, "the algebraic symbol represents an *abstract number*," which, in its turn, is supposed to represent the line.

The third opinion is, that the letter represents not the *number* of units contained in the line, but the *length* of the line itself. Thus, we are told, "the numerical measure of the line may, when known, be substituted at pleasure for the letter which stands for the line; *but it must always be remembered that what the letter denotes is not the number which measures the length, but the length itself.* Thus, if A B (denoted by a) is two inches long, and an inch is the unit of length, we shall have $a = 2$; but if half an inch is the unit, $a = 4$. Here a has two different numerical values, while that which a really represents, the actual length of the line, is in both cases the same."

A a B

Now, if there be no real conflict of views in such diverse teaching, there must certainly be some want

of precision and clearness in the use of language. If the student should confine his attention exclusively to any one of these opinions, he might consider the authors who teach it as quite clear and satisfactory; but if he should extend his researches into other writers on the same subject, he might, perhaps, begin to find that he had something to unlearn as well as something further to learn. He might be made to believe, as thousands have believed, that algebraic symbols can only represent numbers and that, therefore, the only way to bring geometrical magnitudes within the domain of algebraic analysis is to reduce them to numbers by comparing them with their respective units of measure. But, then, if he should happen to see in the work of some celebrated author the still more obvious position laid down that algebraic symbols may be taken to represent magnitudes directly, as well as numbers, it is highly probable he would be disturbed in his former belief. It is likely that he would vacillate between his old conviction and the new idea, and be perplexed. Nor would he be delivered from this unpleasant dilemma on being assured that in Analytical Geometry symbols never denote numbers, but always the undivided magnitudes themselves. Suppose, then, that each of these opinions contains the truth, it is evident that it cannot contain the whole truth, and nothing but the truth, clearly and adequately expressed. On the contrary, the rays of truth they contain are so imperfectly adjusted that, in crossing each other, they produce darkness, perplexity, and confusion in the mind of the student. It is necessary, if possible, so to eliminate and readjust the truths exhibited in these opinions as to avoid all such interference, and all

such darkening of the very first principles of the science.

When it is said that a line is measured by a number, it is evident that an abstract number, such as 2 or 4, cannot be intended. Such numbers represent or measure, not the length of a line, but only the ratio of one line to another. If a line two inches long, for example, be compared with an inch as the unit of measure, the abstract number 2 will be the ratio of this unit to the line, and not "the measure of the line" or its "numerical value." Supposing the line of two inches to be denoted by a, then we shall have, not $a = 2$, but $\frac{a}{1 \text{ inch}} = 2$, or $\frac{2 \text{ inches}}{1 \text{ inch}} = 2$. In like manner, if half an inch be the unit of measure, we shall have, not $a = 4$, but $\frac{2 \text{ inch}}{\frac{1}{2} \text{ inch}} = 4$. In the first case, $a = 2$ inches, and in the second, $a = 4$ half inches, so that, in both cases, we shall have the same value for the same thing, since 2 inches and 4 half inches are *not* "different numerical values."

It should always be remembered that it is only a *denominate number* which truly "measures the length of a line," and that abstract numbers merely represent the ratios of lines. Thus, for example, if a line one yard in length be compared with a foot as the unit of measure, the abstract number 3 will be the ratio of this unit to the line, and if an inch be the unit, then 36 will express this ratio, or the number of times the unit is contained in the line measured. In neither case, however, is 3 or 36 "the numerical measure of the line" or the yard. This is measured, not by the *abstract* number 3 or 36, but by the *denominate* num-

ber 3 feet or 36 inches. Thus, for one and the same thing we have not "two different numerical values," but only one and the same value.

The third opinion, then, appears to have arisen from the supposition that an abstract number, such as 2 or 4, can measure the length of a line, whereas this is always measured by a denominate number. And this being the case, it makes no difference whether the letter be taken to denote the number which measures the length of the line or the length itself. For whether *a* be taken to represent the length itself, as one yard, or the number which measures it, as 3 feet or 36 inches, it will stand for precisely the same magnitude. In one case it will stand for the whole, and in the other for the undivided sum of the parts! Hence, we reject the third opinion as founded on a wrong notion respecting the nature of the number which serves to measure the length of a line, and as being a distinction without a difference.

The second opinion is involved in a similar fallacy. For it proceeds on the assumption that a linear magnitude may be "represented" by an "abstract number;" whereas this can only represent the ratio of one line to another. Indeed, an abstract number bears no relation to the length of a line, and can be brought into relation with it only by means of the unit of measure, either expressed or understood. If, for example, any one were asked how long a particular line is, or how it should be represented, and he were to answer it is three long, or should be represented by 3, he would talk unintelligible nonsense. But if he were to reply it is 3 feet, or 3 miles in length, and should be represented accordingly, he would be understood.

Hence, as abstract numbers do not represent lines, sc the letters which stand for such numbers do not represent them.

There is, then, only one way of representing a line by a letter, and that is by taking the letter to denote the line itself, or, what amounts to the same thing, to denote the *denominate* number which measures the line. This may be done, no doubt, if we please; but is this way of representing lines admissible in Analytical Geometry? It is certainly embarrassed with difficulties which the authors of the second opinion do not seem to have contemplated. If, for example, one line 6 feet long is denoted by a, and another 3 feet long is denoted by b, it is easy to see that $a + b = 9$ feet, $a - b = 3$ feet, and $\frac{a}{b} = 2$; but what shall we say of the product $a\,b$? Or, in other words, of 6 feet by 3 feet? Almost any student, after having gone through with elementary works on pure Geometry or Analytical Geometry, would be ready with the answer—18 square feet. Yet there is no rule in mathematics for the multiplication of one denominate number by another. The product of feet by feet is just as unintelligible as the product of cents by cents; an absurd operation with which some people perplex themselves a great deal to no purpose. The multiplier must always be an *abstract* number. The present writer has often been asked by letter, "What is the product of 25 cents by 25 cents?"—an inquiry as unintelligible as if it were what is the product of 25 cents by 25 apples, or the product of 25 apples by 25 sheep? Such an absurdity would be less frequently committed if elementary works on arithmetic had thrown sufficient

light on the nature of multiplication. But, however obvious this error, it is precisely similar to that committed by geometers when they seek the product of any one concrete magnitude, such as a line or a surface, by another.*

If we would avoid all such errors and difficulties, we must lay aside the notion that magnitudes are represented either directly or indirectly by letters. There is no such representation in the case. Indeed, the rationale or analysis of the whole process of symbolical reasoning lies, as we shall see, beneath this notion of representation, and is something deeper than is usually supposed. Certainly, the abstract number obtained by comparing a line with an assumed unit of length cannot properly be called "the numerical value of the line," as it is by so many authors. For, if it could, then one and the same line might have an infinity of numerical values, since the abstract number would vary with every change in the assumed unit of measure. But surely, if an infinity of numerical values for one and the same thing be not an absurdity in mathematics, it is far too vague and indefinite a notion to find a place in the domain of the most precise and exact of all the sciences.

The precise truth is, that in establishing the theorems of geometry we do not aim to determine the length of lines, but the relations they sustain to each other, as well as to surfaces and solids. In trigonometry, for example, we are concerned, not with the absolute value of the magnitudes considered, but with the relations existing between them; so that when a sufficient number of these magnitudes are

* Note A.

known, or may be measured, the others may be deduced from them by means of the relations they bear to each other. The same is true of all other parts of geometry. Hence, what we need is not a representation of the magnitudes themselves, but of the relations existing between them. We start from certain given relations, we pass on to other relations by means of reasoning; and having found those which are most convenient for our purpose, the theorems of geometry are established and ready for use. The precise manner in which this is accomplished we shall now proceed to explain.

In all our reasoning we deal with abstract numbers alone, or the symbols of abstract numbers. These, it is true, do not, strictly speaking, represent lines or other magnitudes, but the relations between these may, and always should, represent the relations between the magnitudes under consideration. This representation of relations, and not magnitudes, is all that is necessary in symbolical reasoning, and if this be borne in mind the rationale of the whole process may be made as clear as noonday.

The unit of linear measure is altogether arbitrary. It may be an inch, a foot, a yard, a mile, or a thousand miles. But this unit once chosen, the square described on it should be the unit of measure for surfaces, and the cube described on it the unit of measure for solids. Each magnitude, whether a line, surface, or solid, might be compared directly with its own unit of measure, and the abstract number thence resulting might be represented by a single letter. But this course would be attended with much confusion and perplexity. Hence it is far more convenient, and

consequently far more common, to represent only the abstract number obtained from a line by a single letter.

Then will the product of two letters represent the abstract number answering to a surface. Suppose, for example, that the line A B contains 6 units, and the line C D 3 units. Let a denote the abstract number

A ——|——|——|——|——|——| B

C ——|——|——| D

6, and b the abstract number 3, then $a\,b = 18$. Now this product $a\,b$ is not a surface, nor the representative of a surface. It is merely the abstract number 18. But this number is exactly the same as the number of square units contained in the rectangle whose sides are A B and C D, as may be seen, if necessary, by constructing the rectangle. Hence the surface of the rectangle is represented or measured by 18 squares described on the unit of length. This relation is universal, and we may always pass from the abstract unit thus obtained by the product of any two letters to the measure of the corresponding rectangle, by simply considering the abstract units as so many concrete or denominate units. This is what is intended, or at least should always be intended, when it is asserted that the product of two lines represents a surface; a proposition which in its literal sense is wholly unintelligible.

In like manner the product of three letters, $a\,b\,c$, is not a solid obtained by multiplying lines together, which is an impossible operation. It is merely the product of the three abstract numbers denoted by the

letters a, b, and c, and is consequently an abstract number. But this number contains precisely as many units as there are solid units in the parallelopipedon whose three edges are the lines answering to the numbers denoted by a, b, and c; and hence we may easily pass from this abstract number to the measure of the parallelopipedon. We have merely to consider the abstract number as so many concrete units of volume, or cubes described on the linear unit. It is in this sense, and in this sense alone, that the product of three lines, as it is called, represents a solid. Bearing this in mind, as we always should do, we may, for the sake of brevity, continue to speak of one letter as representing a line, the product of two letters as representing a surface, and the product of three letters as representing a solid.

THE COMMONLY RECEIVED DEFINITION OF ANALYTICAL GEOMETRY.

In most definitions this branch of mathematics is exhibited as merely the application of algebra to geometry. Thus, says M. De Fourcy in his treatise on the subject, "*Analytical Geometry*, or in other terms, the *Application of Algebra to Geometry*, is that important branch of mathematics which teaches the use of algebra in geometrical researches." This definition, like most others of the same science, can impart to the beginner no adequate idea of the thing defined. It fails in this respect, partly because the geometrical method used in this branch of mathematics is different from any with which his previous studies have made him acquainted, and partly because algebra itself undergoes an important modification in its application to

this new geometrical method. These points must be cleared up and the student furnished with new ideas before he can form a correct notion of Analytical Geometry.

According to the above definition, no new method, no new principle is introduced by Analytical Geometry; it is simply the use of algebra in geometrical investigations. Precisely the same idea underlies nearly all definitions of this branch of mathematics. Thus, in one of these definitions, we are told that "in the application of *algebra to geometry*, usually called *Analytic Geometry*, the magnitudes of lines, angles, surfaces, and solids are expressed by means of the letters of the alphabet, and each problem being put into equations by the exercise of ingenuity, is solved by the ordinary processes of algebra." In another it is said that "Analytical Geometry" is that "branch of mathematics in which the magnitudes considered are represented by letters, and the properties and relations of these magnitudes are made known by the application of the various rules of algebra." Now these definitions, and others which might be produced, convey no other idea of the science in question than that it is the use of algebraic symbols and methods in geometrical researches. They contain not the most distant allusion to that new and profoundly conceived geometrical method, nor to that peculiar modification of algebra, by the combination of which Analytical Geometry is constituted.

This beautiful science, it is universally conceded, was created by Descartes. But if the above definition be correct, then Vieta, and not Descartes, was the creator of Analytical Geometry; for he made precisely

such use of algebra in his geometrical researches. Indeed, we could not better describe the method of Vieta than by adopting some one of the current definitions of Analytical Geometry. It is most accurately exhibited in the following words of a recent author: "There are three kinds of geometrical magnitudes, viz., lines, surfaces, and solids. In geometry the properties of these magnitudes are established by a course of reasoning in which the magnitudes themselves are constantly presented to the mind. Instead, however, of reasoning directly upon the magnitudes, we may, if we please, represent them by algebraic symbols. Having done this, we may operate on these symbols by the known methods of algebra, and all the results which are obtained will be as true for the geometrical quantities as for the algebraic symbols by which they are represented. This method of treating the subject is called Analytical Geometry." Now every word of this description is accurately and fully realized in the labors of Vieta. Hence, if it had been given as a definition of the "Application of Algebra to Geometry," as left by him, it would have been free from objection. But it cannot be accepted as a definition of Analytical Geometry. For such method, however valuable in itself, is not Analytical Geometry, nor even one of its characteristic properties. It is not that grand era of light by which the modern geometry is separated from the ancient. For that era, or the creation of Analytical Geometry, is, according to the very author of the above definition himself, due to Descartes. Yet his definition of Analytical Geometry, like most others, includes the method of Vieta, and excludes the method of its acknowledged author, Descartes.

Even before the time of Vieta, Regiomontanus, Tartaglia, and Bombelli solved problems in geometry by means of algebra. But in each case they used numbers to express the known lines and letters to represent the unknown ones. Hence their method was confined within narrow limits when compared with the method of Vieta. He was the first who employed letters to represent known as well as unknown quantities; a change, says Montucla, "to which algebra is indebted for a great part of its progress." It enabled Vieta and his successors to make great advances in Geometry as well as in algebra. But it did not enable him to reveal or to foresee the new method which was destined to give so mighty an impulse to the human mind, and produce so wonderful a revolution in the entire science of mathematics, whether pure or mixed. This was reserved for the transcendent genius of Descartes.

It seems to me that a definition of Analytical Geometry should include the method of Descartes (its acknowledged author) and exclude that of Vieta. It is certain that if we should adopt the above definition, we should be compelled to include Trigonometry, as well as "the solution of determinate problems," in Analytical Geometry. Indeed, M. De Fourcy, after having defined Analytical Geometry as above stated, expressly adds, "Under this point of view it ought to comprehend trigonometry, which forms the first part of this treatise." In like manner, Biot, Bourdon, Lacroix, and other French writers, embrace trigonemetry, as well as "the solution of determinate problems," in their works on Analytical Geometry. This seems to be demanded by logical consistency, or a

strict adherence to fundamental conceptions, since analytical trigonometry, no less than determinate problems of geometry, is clearly included in their definitions. We shall exclude both, because neither comes within the definition which we intend to adopt. Trigonometry and determinate problems of geometry were, indeed, both treated by means of algebra long before Analytical Geometry, properly so called, had an existence or had been conceived by its great author.

Those American writers, however, who have adopted the above definition, exclude trigonometry, though not the solution of determinate problems, from their works on Analytical Geometry. Hence, in excluding both, the additional omission will be very slight, inasmuch as the solutions of determinate problems in the works referred to constitute only a few pages. These few pages, too, being little more than a mere extension of ordinary algebra, should, it seems to me, form a sequel to that branch of mathematics, rather than a heterogeneous prefix to Analytical Geometry. It is certain that by such a disposition of parts we should restore an entire unity and harmony of conception to the beautiful method of Descartes, by which a new face has been put on the whole science of mathematics.

This method and that of Vieta are, as M. Biot says, "totally separated in their object." Hence he was right in determining, as he did, "to fix precisely and cause to be comprehended this division of the Application of Algebra to Geometry into two distinct branches," or methods of investigation. Since these two branches, then, are so "totally separated in their objects," as well as in their methods, we shall separate them in our definitions. We certainly shall not, in

our definition, cause the method of Vieta to cover the whole ground of Analytical Geometry, to the entire exclusion of the method of Descartes. The method of Vieta is, indeed, nowhere regarded as constituting Analytical Geometry, except in the usual definitions of this branch of mathematics. The authors of these definitions themselves entertain no such opinion. On the contrary, they unanimously regard the method of Descartes as constituting Analytical Geometry, though this view is expressed elsewhere than in their definitions. Thus, after having disposed of "determinate problems," and come to those investigations which belong to the Cartesian method, one of these authors adds in a parenthesis, "*and such investigations constitute the science of Analytical Geometry.*" If so, then surely the nature of such investigations should not have been excluded, as it has been, from his definition of this branch of mathematics. In like manner, another of these authors, after having discussed the subject of determinate problems, enters on the method of Descartes with the declaration that this philosopher by his great discovery "really created the science of Analytical Geometry." Why then was this great discovery excluded from his definition of the science? In spite of their definitions, we have, indeed, the authority of these writers themselves that Analytical Geometry, properly so called, is constituted by something different and higher and better than the algebraic solutions of determinate problems of geometry.

THE OBJECT OF ANALYTICAL GEOMETRY.

In order to unfold in as clear and precise a manner as possible the great fundamental conceptions of Analytical Geometry, we shall consider first, the object of the science; and, secondly, the means by which this object is attained.

"Geometrical *magnitudes*, viz., lines, surfaces, and solids," are, it is frequently said, the objects of Analytical Geometry. But this statement can hardly be accepted as true. For lines, surfaces, and solids, considered as magnitudes, are not, properly speaking, the objects of this science at all. *Lines* and *surfaces*, it is true, as well as points, are considered in Analytical Geometry; but then they are discussed with reference to their *form* and *position*, and not to their magnitude. Questions of *form* and *position* are those with which Analytical Geometry, as such, is chiefly and pre-eminently conversant. So long, indeed, as our attention is confined to questions of magnitude, whether pertaining to lines, surfaces, or solids, we are in the domain of the old geometry. It is the peculiar province and the distinctive glory of the new that it deals with the higher and more beautiful questions of *form*.

In relation to the discoveries of Descartes in mixed analysis, Montucla says, "That which holds the first rank, and which is the foundation of all the others, is the application to be made of algebra to the geometry of curves. We say to the *geometry of curves*, because we have seen that the application of algebra to ordinary problems is much more ancient." If we would obtain a correct idea of his method, then, we must lay aside as unsuited to our purpose the division of geo-

metrical magnitudes into lines, surfaces, and solids. For however important this division or familiar to the mind of the beginner, it is not adapted to throw light on the nature of Analytical Geometry. If we would comprehend this, we must divide all our geometrical ideas into three classes—namely, into ideas of *magnitude*, *position*, and *form*. Of these the most easily dealt with are ideas of magnitudes, because magnitudes, whether lines, surfaces, or solids, may be readily represented by algebraic symbols.

Indeed, to find a "geometrical locus," or, in other words, to determine the form of a line, was the unsolved problem bequeathed by antiquity to Descartes, and with the solution of which he bequeathed his great method to posterity. Thus the new geometry had its beginning in a question of form, and, from that day to this, all its most brilliant triumphs and beautiful discoveries have related to questions of form. These high questions, it is true, his method brings down to simple considerations of magnitude, or, more properly speaking, the relations of linear magnitudes. The objects it considers are not *magnitudes;* they are *forms* and the *properties* of form. The magnitudes it uses and represents by letters are only auxiliary quantities introduced to aid the mind in its higher work on forms. They are the scaffolding merely, not the edifice. In what manner this edifice, this beautiful theory of the ideal forms of space, has been reared by Analytical Geometry, we shall now proceed to explain.

THE GEOMETRIC METHOD OF ANALYTICAL GEOMETRY.

The new geometry consists, as we have said, of a geometric method and a modified form of algebra. Both of these should, therefore, be embraced in its definition. We begin with an explanation of its geometric method.

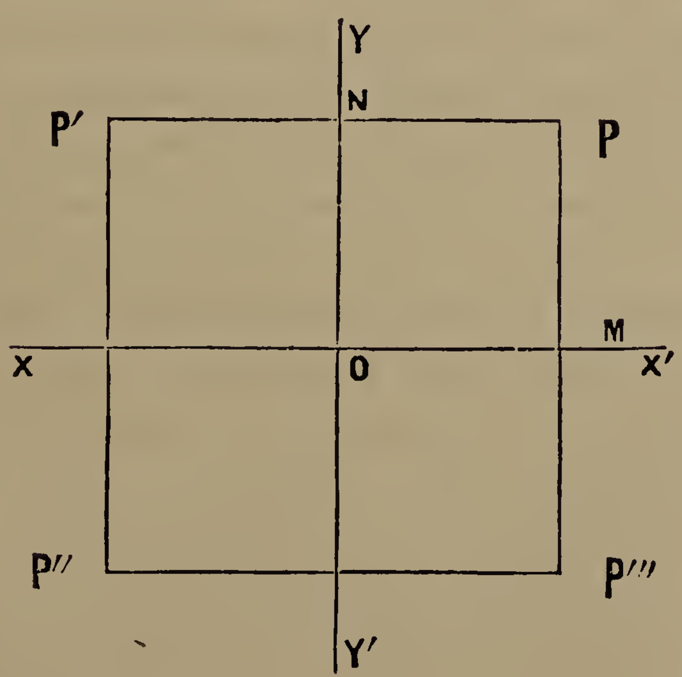
"By co-ordinate geometry," says Mr. O'Brien, "we mean that method or system invented by Descartes, in which the position of points are determined and the forms of lines and surfaces defined and classified by means of what are called co-ordinates." This appears to be a correct definition of the system of Descartes; at least in so far as its geometric method is concerned. But that method, as we shall see, however important as an integral portion of the system, is barren in itself, and becomes fruitful only by a union with the analytic method of the same system. Adopting, for the present, the above definition as applicable to the geometric method of Descartes, it remains for us to unfold and illustrate its meaning.

It is easy to see that every question of form depends on one of position, since the form of any line or surface is constituted by the position of its various points. If, then, the position of every point of a line (to begin with the more simple case) be determined, it is clear that the form of the line will be fixed. Hence the first step in the system of Descartes, or in the modern doctrine of form, is the method by which it determines the position of a point in a plane.

THE METHOD OF DETERMINING THE POSITION OF A POINT.

From time immemorial the position of a point on the surface of the earth has been determined by its distance from two fixed lines—namely, an assumed meridian and the equator. These two distances, known as the longitude and latitude of the point or place, are among the most natural and easy means by which its position can be fixed. Yet this method, although so natural, so simple, and so familiar in practice, lay upon the very surface of things for many centuries before its immense scientific value began to be apprehended. Descartes, in the seventeenth century, was the first philosopher by whom it was adopted into geometry, generalized, and made to impart incalculable new resources to the science.

In order to fix the position of a point on a plane, we trace in the plane, in conformity with the method of Descartes, two right lines X X′ and Y Y′, which



make a given angle (usually a right angle) with each other, and we draw through the point P, whose position is to be determined, parallel to these lines, the two right lines P M and P N, cutting them in the points M and N. Now it is evident that the point P will be determined when we know the points M and N, for we can draw through these points M P parallel to O Y, and N P parallel to O X′, and these parallels will intersect in the point P. But the point M is determined when we know the distance O M, and the point N when we know the distance O N. Hence the point P is determined or fixed by means of the distance O M or its equal N P, and the distance O N or its equal M P. That is, on the supposition that P lies in the angle Y O X′; otherwise its position could not be fixed by these magnitudes alone.

For were these magnitudes O M and O N given, this would not serve to determine the point to which they answer, since there are four points P, P′, P″, and P‴, all of which answer to precisely the same magnitudes or distances. To avoid the confusion which must have resulted from such uncertainty of position, Descartes adopted a very simple and efficient artifice. Instead of employing a different set of letters for each of the angles in which the required point might be found, he effected his object and cleared away every obscurity by the simple use of the signs + and —. That is, he represented the magnitudes O M and O N by the same letters, and they were made to determine the point P, P′, P″, or P‴, according to the signs attached to these letters.

Thus, for example, O M is represented by a and O N by b; when a is plus, it is laid off in the direction

from O toward X′, and when it is minus it is measured in the opposite direction, or from O toward X. In like manner, when b is plus, it is counted from O toward Y, or above the line X X′, and when it is minus, in the opposite direction, or from O toward Y′. What is thus said of the distances O M and O N, or their representatives a and b, is applicable to all similar distances. Thus, by the use of two letters and two signs, the position of any point in any one of the four infinite quarters of the plane is indicated without the least uncertainty or confusion.

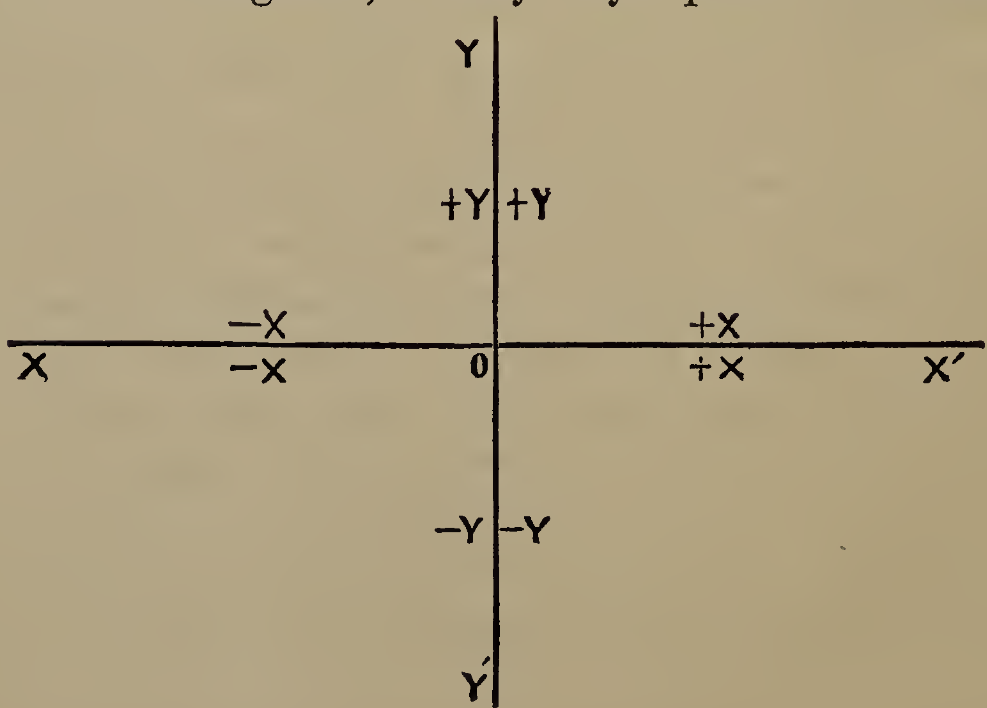
The distance O M, or its equal N P, is called the *abscissa*, and the distance O N or its equal M P, is called the *ordinate* of the point P to which they answer. These distances when taken conjointly are denominated the *co-ordinates* of the point. Instead of saying the point whose abscissa is denoted by a and ordinate by b, we simply say, the point (a, b). The line X X′, on which the abscissas are laid off, is called the axis of abscissas, and the line Y Y′ the axis of ordinates. Both together are denominated the *co-ordinate axes*. The point O, in which the co-ordinate axes intersect, is known as the *origin of co-ordinates*; or more briefly, as *the origin*.

As the object is to determine, not the absolute but only the relative position of points, so the *co-ordinate axes*, or lines of reference, may be assumed at pleasure. We may place the origin, or incline the axes, so as to meet the exigencies of any particular case, or to answer any special purpose. In general, however, it is more convenient to refer points to axes which make right angles with each other; in which case the system of co-ordinates is *rectangular*. If they are in-

clined to each other, then they form an *oblique* system of co-ordinates. The former, or the rectangular system of co-ordinates, should always be understood, unless it be otherwise expressed.

In the foregoing remarks we have spoken of the point P which is supposed to remain fixed, and whose co-ordinates a and b are therefore constant. But suppose this point, or any other, to move on the plane of the co-ordinate axes, it is evident that its co-ordinates will no longer remain constant or unchanged. On the contrary, as the point moves, either one or both of its co-ordinates must undergo corresponding changes of value. These variable co-ordinates, answering to all the positions of the movable point, or to all the points of the line it describes, are in general denoted by the letters x and y, and the line X X′ on which the abscissas are measured is sometimes called the axis of x, and the line Y Y′, on which ordinates are laid off, the axis of y.

As x and y may assume all possible values, whether positive or negative, so they may represent the co-or-



dinates of any point in the plane of the axes. The angle to which the point belongs will depend, as we have seen, on the algebraic signs of its co-ordinates x and y. By means of the preceding diagram we may perceive at a glance the angle to which any point belongs when the signs of its co-ordinates are known.

Thus we always have

x positive and y positive for the angle Y O X′,
x negative and y positive for the angle Y O X,
x negative and y negative for the angle Y′O X,
x positive and y negative for the angle Y′O X′.

In Analytical Geometry, then, the letters x and y represent not unknown, determinate values or magnitudes as in algebra, but variable quantities. It is this use of variable co-ordinates and symbols of indetermination to represent them which constitutes the very essence of the Cartesian system of geometry—a system of whose analytic portion, however, we have as yet caught only an exceedingly feeble glimpse. It justly claims, in this place, a somewhat fuller exposition, especially since its value is so completely overlooked in the definitions of most writers on the subject. Even the definition of Mr. O'Brien contains, as we have seen, only the geometrical method of Descartes, and not the most distant allusion to its analytic method. Indeed, in his preface, this author asserts that the subject of which he treats "is usually styled *Analytical Geometry*, but its real nature seems to be the better expressed by the title Co-ordinate Geometry, since *it consists entirely in the application of the method of co-ordinates to the solution of geometrical problems.*" Yet this method of co-ordinates, if separated from the method of indeterminate analysis, can

conduct us but a very little way on the high road of Analytical Geometry, and even that little way it must proceed with extreme slowness and difficulty. Analytical Geometry should, it seems, be at least styled *the application of indeterminate algebraic analysis to the geometrical method of co-ordinates.* We shall now proceed to justify this opinion, and show that the science in question does not "entirely consist" in the application of the geometrical method.

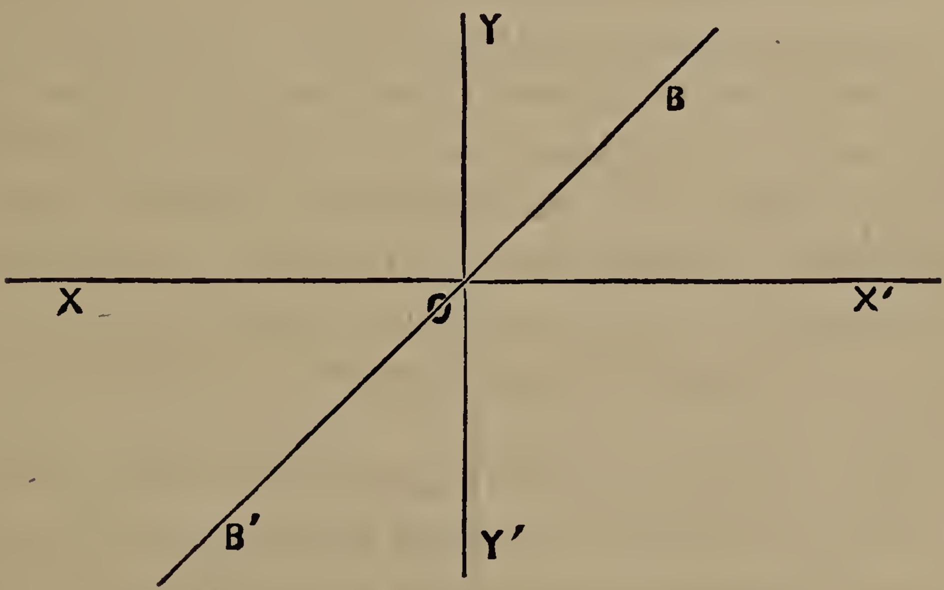
THE ANALYTIC METHOD OF ANALYTICAL GEOMETRY.

The first step of this method consists in the analytic representation of geometrical forms. If a point move at random on the plane of the axes, it is evident that its variable co-ordinates will be wholly independent of each other, and consequently no relation can be established between them. The line or curve thus described by a point moving without law or order does not come within the domain of Analytical Geometry.

There is, however, an infinity of lines which, instead of being described by a point moving at random, are traced according to some fixed law or invariable order. In such cases there exists between the abscissa and ordinate of each point of the line a constant relation or unchanging mutual dependence. In passing from one point of such line to another, the abscissa and ordinate must change, but the relation between them may remain the same. In a great variety of instances this uniform relation is such that it may be expressed by an equation between x and y, the symbols of the co-ordinates so related to each other. *The equation which thus expresses the relation between the abscissa and*

ordinate of each point of a line is called the equation of the line. The line, in its turn, is called *the locus of the equation;* but it is still more frequently called the *locus of the point* by which it is described.

The equation of a line once formed will enable us, by suitable operations upon it, to detect all the circumstances and to discover all the properties of the line or locus to which it belongs. A few simple illustrations will serve to put this great fundamental truth in a clearer light. Suppose, then, that there is a right line, such as B B′, which divides the angle Y O X′ into



two equal parts, then it is clear that its ordinate will always be equal to its abscissa. The abscissa may assume all possible values from zero to plus infinity; yet through all its changes it will remain constantly equal to the corresponding ordinate. This invariable relation between the two variable co-ordinates is perfectly expressed or represented by the equation

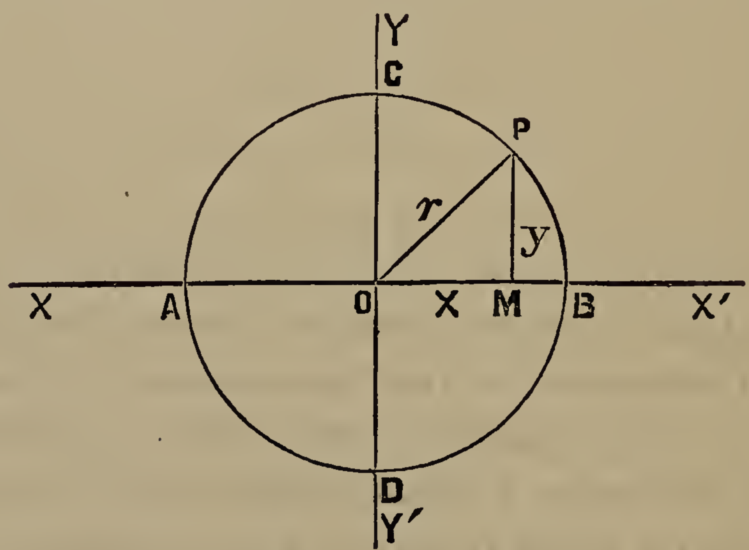
$$y = x,$$

which is therefore the equation of the line B B′. In

like manner, *the equation of any line institutes precisely the same relation between the symbols x and y as that which exists between the co-ordinates symbolized or represented by them.*

If in this equation we give positive values to x, we shall find positive values for y, and these values will determine points of the right line in the angle Y O X′. If we give negative values to x, we shall then have negative values for y, and these values will determine points of the line in the angle Y′ O X. But, leaving this most simple of all cases, let us pass on to still more interesting examples.

Let it be required, then, to find the equation of the circumference of a given circle, and show how this equation may be made to demonstrate some of the properties of that curve. We place the origin of co-ordinates at the centre of the given circle A C B D,



and denote its known radius by the letter r. Then, for any point of the circumference, as P, the square of the ordinate, plus the square of the abscissa, is equal to the square of the radius, since the sum of the squares on the sides of a right-angled triangle is equal to the

square of its hypothenuse. This relation is expressed by the equation

$$y^2 + x^2 = r^2,$$

and as this is true for every point of the circumference, so this is the equation of the curve. Now from this equation all the properties of the circumference of the circle may be evolved by suitable transformations.

As our present purpose is merely illustrative, we shall, in this place, evolve only a single property. Then the equation gives

$$y^2 = r^2 - x^2$$

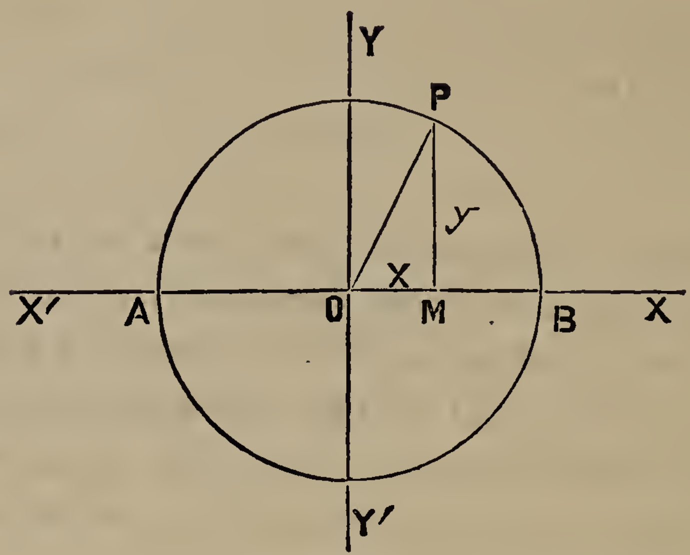
$$y^2 = (r + x)(r - x);$$

but $r + x = \text{A M}$; $r - x = \text{M B}$; and $y = \text{P M}$; hence

$$\text{P M}^2 = \text{A M} \times \text{M B};$$

that is, *the perpendicular let fall from a point of the circumference of a circle to its diameter is a mean proportional between the segments into which it divides the diameter*—a well-known property of the circle.

Or, if we choose, we may set out from this property, and, putting it into an equation, deduce therefrom the ordinary definition of the circumference of a circle. Thus, let it be required to prove that the line whose ordinate is always a mean proportional between the segments into which it divides a given distance on the axis of X, is the circumference of a circle; or, in other words, is everywhere equidistant from a certain point in the plane of the axes. We suppose A B, the given distance, $= 2r$, and we place the origin of co-ordinates O at its middle point, so that $\text{A O} = \text{O B} = r$. Then,



if P be any point on the line in question, we shall have from its definition

$$PM^2 = AM \times MB;$$

but $PM = y$; $AM = r + x$; and $MB = r - x$; hence, by substitution, $y^2 = (r + x)(r - x) = r^2 - x^2$,

or $$y^2 + x^2 = r^2;$$

but $$y^2 + x^2 = OP^2,$$

therefore $$OP^2 = r^2, \text{ or } OP = r.$$

That is to say, any point of the line in question is always at the same distance r from the origin of co-ordinates; in other words, it is the circumference of a circle whose centre is in the middle of the line A B. Indeed, this might have been inferred from its equation $y^2 + x^2 = r^2$, which is that of the circle whose centre is at the origin. What we have thus shown so very partially in regard to the circumference of a circle is equally true of other curved lines discussed in Analytical Geometry. That is, we may set out from any one of their properties as a definition, and putting this

into an equation, deduce its other properties therefrom by suitable transformations of its equation.

We shall add one more illustration. Suppose the question, for example, be to determine the principal circumstances of position and form of the line, the square of whose ordinate varies as the corresponding abscissa; or, in other words, the square of whose ordinate is always equal to the rectangle of the abscissa into some constant line, as $2p$. The equation of this line is simply the analytic statement of its definition, and is

$$\frac{y^2}{x} = 2p,$$

or

$$y^2 = 2px.$$

Now if, in the first place, we wish to find the point in which the line cuts the axis of X, we must determine the co-ordinates of that point, since every point is made known by its co-ordinates. But as the required point lies on the axes of x, it is evident that its ordinate is o, and this, substituted for y in the equation, gives

$$o = 2px; \text{ or } x = o$$

for the corresponding value of the abscissa. Hence the line cuts the axis of X at the origin of co-ordinates, since that is the only point whose co-ordinates are both o.

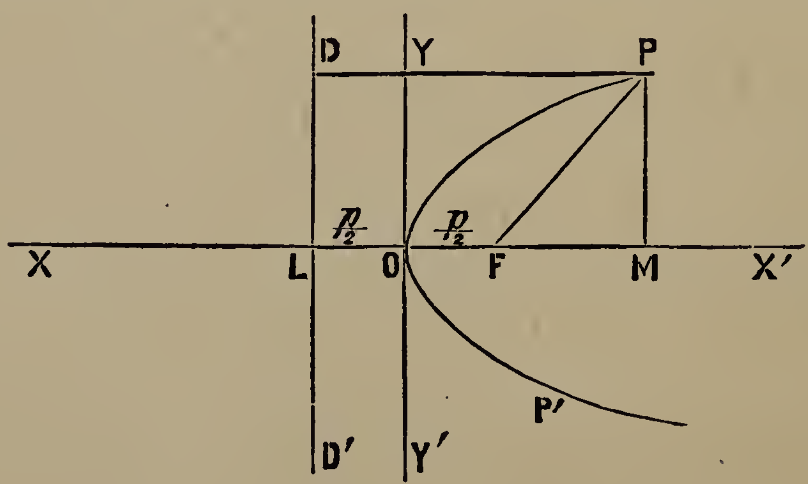
Again, we put the equation in this form:

$$y = \pm \sqrt{2px},$$

from which we see that if x be minus, then y will be imaginary; or, in other words, there will be no corres-

ponding value of the ordinate. That is to say, there is no point of the curve answering to a negative value of x, or to the left of the axis Y Y′. But if we assign any positive value to x, there will be two equal values for y; the one plus and the other minus. Hence there are two points of the curve answering to each positive value of x; the one above and the other below the axis of X, and at equal distances from it. As this is true for every value of x, though it may vary from O to plus infinity, it appears that the two branches of the curve are unlimited on the right side of the axis Y Y′, or proceed to infinity.

If we give any particular value to x, as $x = 4$, then we shall have $y = \pm \sqrt{2p \cdot 4} = \pm 2\sqrt{2p}$; and by means of these known pairs of co-ordinates, we may determine two points of the line—one answering to the plus, and the other answering to the minus, value of y. In like manner we might determine and mark any number of points belonging to the locus of the equation $y = \pm \sqrt{2px}$. By pursuing this method we should discover that the locus in question is a curve, whose general form P O P′ is represented in the diagram below. But this would be an exceedingly tedi-



ous method, and would require immense calculations to determine the curve with any degree of accuracy. It is, indeed, the method of co-ordinates, and serves to illustrate the imperfections of that method when unaided by the higher powers of the analytic portion of the Cartesian system.

By calling this to our aid, we may easily discover a property of the line in question which will enable us to describe it by a continuous motion without the necessity of such tedious or operose calculations. Thus, if we lay off $OF = \frac{p}{2}$, and $OL = \frac{p}{2}$, and through the point L erect an indefinite perpendicular DD' to the axis XX', then each and every point of the curve in question will enjoy this remarkable property — namely, *it will be at an equal distance from the point* F *and the line* DD'.

That is, if from any point, as P, of the curve, a right line PF be drawn to F, and another perpendicular to DD', then $PF = PD$. For

$$PF^2 = PM^2 + FM^2 = y^2 + (x - \frac{p}{2})^2,$$

or $$PF^2 = y^2 + x^2 - px + \frac{p^2}{4};$$

but $$y^2 = 2px;$$

hence, $$PF^2 = x^2 + px + \frac{p^2}{4} = (x + \frac{p}{2})^2,$$

or $$PF = x + \frac{p}{2};$$

but $$PD = x + \frac{p}{2};$$

therefore,

$$PF = PD,$$

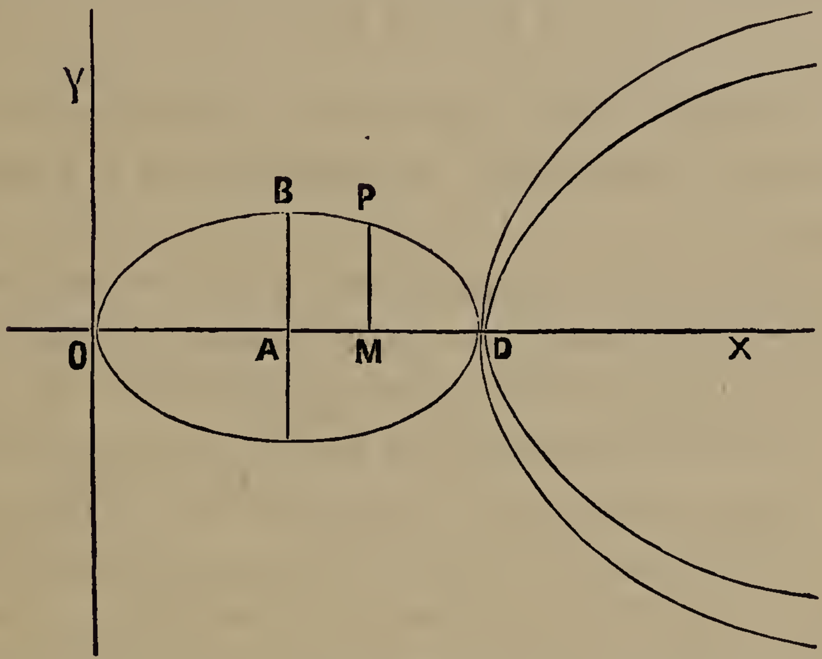
as enunciated. This remarkable property enables us to describe the curve in question by a continued motion.

The above very simple illustrations, or instances of discovery, are but mere scintillations of that great analytic method which seems as inexhaustible as the sun, and which has already poured floods of light on every department of the exact sciences. The geometrical method itself, however important, is chiefly valuable as a basis for this analytic method. The two methods are, however, indispensable to each other, and it was by the happy union of both that Analytical Geometry was created. It was, moreover, by the wonderful fecundity and power of this combination that the way was opened for the discovery of the Infinitesimal Calculus, and for the solution of the grand problems of the material universe, as well as for the renovation and reconstruction of all the physical sciences.

The great beauty of this method consists in the generality of its solutions—a generality which is capable of being rendered far greater than is usual in works on Analytical Geometry. To illustrate this point: *Let it be required to find the equation of a curve such that the square of any ordinate shall be to the rectangle of the distances between its foot and two fixed points on the axis of x in a given ratio.*

Let O and D be the two fixed points on the axis of

x, and let the distance between them, O D, be denoted by $2a$; take the origin of co-ordinates at O, and suppose the given ratio is $b^2 : a^2$, in which b represents



any line whatever. At the point A, the middle of the line O D, erect the perpendicular $AB = b$, and let P be any point of the curve whose equation is required. O M is the abscissa, and P M is the ordinate of that point, and the two distances between its foot, M, and the two fixed points O and D are M D and O M. Hence, by the condition of the problem, we have

$$PM^2 : OM \times MD :: b^2 : a^2,$$

or

$$y^2 : x(2a - x) :: b^2 : a^2;$$

since P M, for any point of the curve, is the variable ordinate y, and O M the variable abscissa x, and since $MD = OD - OM = 2a - x$.

Hence
$$y^2 = \frac{b^2}{a^2}(2ax - x^2)$$

is the equation of the curve required.

Now the whole folio of Apollonius, in which he discusses with such wonderful ability the conic sections, is wrapped up and contained in this one equation. For, by the discussion of this one equation, we may easily ascertain the form and all the other properties of the circle, of the ellipse, of the hyperbola, and of the parabola;* unfolding from one short analytic expression the whole system of beautiful truths which caused Apollonius to be regarded as the greatest of all the geometers of the ancient world, except Archimedes.

The method of *Descartes* consisted in the happy use of a system of auxiliary variables, such as x and y, representing the variable co-ordinates of a series of points. In addition to these variables, Newton and Leibnitz employed another system composed of the variable increments or decrements which x and y undergo in passing from one point to another; or, in other words, the variable differences of the variable co-ordinates. Thus, the systems of these two illustrious geometers were both erected on the foundation which Descartes had laid, and which had introduced so wonderful a revolution into the whole science of mathematics. It has been well said, then, that "*Descartes* not only perfected the work of *Vieta*, but he also invented methods at once simple and fruitful, in order to bring the theory of curves within the grasp of the algebraic analysis, and these methods are, in the eyes of posterity, the most beautiful title to glory of that celebrated philosopher."†

* Any one who is master of the fundamental idea of *Descartes* may easily do this; it is done in my unpublished work on Analytical Geometry.

† See Note B.

THE METHOD OF INDETERMINATES.

Descartes approached the differential calculus in more directions than one. "It seems to me," says Carnot, "that Descartes, by his method of indeterminates, approached very near to the infinitesimal analysis; or rather, it seems to me, that the infinitesimal analysis is no other than the happy application of the method of indeterminates."

"The fundamental principle of the method of indeterminates, or of indeterminate co-efficients, consists in this, that if we have an equation of the form

$$A + B\,x + C\,x^2 + D\,x^3 + \text{etc.} = 0,$$

in which the co-efficients A, B, C, etc., are constant, and x a small quantity, which can be supposed as small as we please, it necessarily follows that each of these co-efficients taken separately must be equal to zero; that is to say, that we shall always have

$$A = 0,\ B = 0,\ C = 0,\ \text{etc.},$$

whatever may be the number of the terms of the equation.

"Indeed, since we can suppose x as small as we please, we can also render as small as we please the sum of all the terms which has x for its factor; that is to say, the sum of all the terms which follow the first. Then that first term A differs as little as we please from 0; but A being a constant cannot differ as little as we please from 0, since then it would be a variable, then it can be only 0, then we have $A = 0$; there remaining thus:

$$\mathrm{B}\,x + \mathrm{C}\,x^2 + \mathrm{D}\,x^3 + \text{etc.} = 0.$$

I divide the whole by x, and I have

$$\mathrm{B} + \mathrm{C}\,x + \mathrm{D}\,x^2 + \text{etc.} = 0,$$

from which we deduce $\mathrm{B} = 0$ by the same reasoning that we have given to prove $\mathrm{A} = 0$; the same reasoning would likewise prove $\mathrm{C} = 0$, $\mathrm{D} = 0$, etc.

"That granted, let there be an equation with only two terms—

$$\mathrm{A} + \mathrm{B}\,x = 0,$$

in which the first term is constant and the second susceptible of being rendered as small as we please; that equation cannot subsist after what has been said, unless A and $\mathrm{B}\,x$ are each in particular equal to zero. Then we may establish this as a general principle, and as an immediate corollary from the method of indeterminates, that *if the sum or the difference of two pretended quantities is equal to zero, and if the one of the two can be supposed as small as we please, while the other contains nothing arbitrary, then the two pretended quantities will be each in particular equal to zero.*"

"This principle alone suffices for the resolution by ordinary algebra of all the questions which belong to the infinitesimal analysis. The respective procedures of the one and of the other methods, simplified as they ought to be, are absolutely the same; all the difference is in the manner of considering the question; the quantities which are *neglected* in the one as infinitely small are *unexpressed* in the other, though considered as finite, because it is demonstrated that they ought to eliminate themselves by themselves, that

is to say, to destroy one another in the result of the calculus.

"Indeed, it is easy to perceive that the result can be only an equation with two terms of which each in particular is equal to zero; we can then suppress beforehand, in the course of the calculus, all the terms of these two equations of which we do not wish to make use. Let us apply this theory of indeterminates to some examples.

"For a second example,"* says the author, "let us propose to prove that the area of a circle is equal to the product of its circumference by the half of its radius; that is to say, denoting the radius by R, the ratio of the circumference to the radius by π, and consequently that circumference by π R, S the surface of the circle, we ought to have

$$S = \tfrac{1}{2}\pi R^2.$$

"In order to prove this I inscribe in the circle a regular polygon, then I successively double the number of its sides until the area of the polygon differs as little as we please from the area of the circle. At the same time the perimeter of the polygon will differ as little as we please from the circumference π R, and the apothem as little as we please from the radius R. Then the area S will differ as little as we please from the $\frac{1}{2}\pi R^2$; then if we make

$$S = \tfrac{1}{2}\pi R^2 + \rho,$$

the quantity ρ, if it is not zero, can at least be supposed as small as we please. That supposed, I put the equation under the form

* The first example is quite too long for my purpose, and besides, it would not be understood by the reader without a knowledge of what had gone before in the work of Carnot.

$$(S - \tfrac{1}{2}\pi R^2) - \rho = 0,$$

an equation of two terms, the first of which contains nothing arbitrary, and the second of which, on the contrary, can be supposed as small as we please; then, by the theory of indeterminates, each of these terms in particular is equal to 0, then we have

$$S - \tfrac{1}{2}\pi R^2 = 0, \text{ or } S = \tfrac{1}{2}\pi R^2;$$

which was to be demonstrated.

"Let it be proposed now to find the value which it is necessary to give to x, in order that its function $a x - x^2$ may be a *maximum*.

"The case of a *maximum* ought evidently to have place, when by adding to the indeterminate x an arbitrary increment x', the ratio of the corresponding augmentation of the proposed function $a x - x^2$ to x' can be rendered as small as we please by diminishing x' more and more.

"But if I add to x the quantity x', I shall have for the augmentation of the proposed function

$$a(x + x') - (x + x')^2 - (a x - x^2),$$

or by reducing

$$(a - 2x)x' - x'^2,$$

it is then the ratio of this quantity to x', or

$$a - 2x - x',$$

which we ought to have the power to suppose as small as we please. Let this quantity $= \rho$, we shall then have

$$a - 2x - x' = \rho,$$

or $$(a - 2x) - (x' + \rho) = 0,$$

an equation of two terms, the first of which contains nothing arbitrary, and the second of which may be supposed as small as we please; then by the theory of indeterminates, each of these terms taken separately is equal to 0. Then we have

$$a - 2x = 0, \text{ or } x = \tfrac{1}{2}a,$$

which was required to be found.

"Let it be proposed to prove that two pyramids with the same bases and the same heights are equal to each other.

"Consider these pyramids divided into the same number of frustums, all of the same height. Each of these frustums may evidently be regarded as composed of two parts, the one of which will be a prism having for its base the smaller of the two which terminates the frustum, and the other will be a sort of aglet which surrounds that prism.

"If, then, we call V, V′, the volumes of the two pyramids, P, P′, the respective sums of the prisms, of which we have just spoken, q, q', the respective sums of the aglets, we shall have

$$\mathrm{V} = \mathrm{P} + q, \ \mathrm{V}' = \mathrm{P}' + q',$$

or $$\mathrm{V} - q = \mathrm{P}, \ \mathrm{V}' - q' = \mathrm{P}'$$

But it is clear that $\mathrm{P} = \mathrm{P}'$, then

$$\mathrm{V} - q = \mathrm{V}' - q', \text{ or } (\mathrm{V} - \mathrm{V}') - (q - q') = 0.$$

But the first term of this equation contains nothing arbitrary, and the second can evidently be supposed as

small as we please. Then, by the theory of indeterminates, each of these terms in particular is equal to zero. Then we have

$$V - V' = 0, \text{ or } V = V',$$

which was to be demonstrated."*

After proving, by the same method, that the volume of a pyramid is equal to one-third of its base by its altitude, and showing that the process is identical with that of the calculus, Carnot adds: "We then see that the method of indeterminates furnishes a rigorous demonstration of the infinitesimal analysis, and that it gives at the same time the means of supplying its place, if we wish, by the ordinary analysis. It is desirable, perhaps, that the differential and integral calculus had been arrived at by this route, which was as natural as the road that was actually taken, and would have prevented all the difficulties."† But however ingenious or striking such application of the method of indeterminates, if Carnot had only tried that method a little further, he would have found that it is an exceedingly poor substitute for the differential and integral calculus. For these, in fact, grapple successfully with an infinity of difficult questions which the method of indeterminates is wholly unable to solve.

* Reflexions, etc., Chapter III.

† Ibid.

CHAPTER VI.

THE METHOD OF LEIBNITZ.

"LEIBNITZ who was the first," says Carnot,* "to give rules for the infinitesimal calculus, established it upon the principle that we can take at pleasure, the one for the other, two finite magnitudes which differ from each other only by a quantity infinitely small. This principle had the advantage of an extreme simplicity and of a very facile application. It was adopted as a kind of axiom, and he contented himself with regarding these infinitely small quantities as quantities less than those which can be appreciated or seized by the imagination. Soon this principle operated prodigies in the hands of Leibnitz himself, of the brothers Bernouilli, of de L'Hôpital, etc. Still it was not free from objection; they reproached Leibnitz (1) with employing the expression infinitely small quantities without having previously defined it; (2) with leaving in doubt, in some sort, whether he regarded his calculus as absolutely rigorous, or as a simple method of approximation."†

* Reflexions, etc., Chapter I., p. 36.

† This objection to the calculus is two hundred years old; it has always arisen, naturally, if not necessarily, in view of the fact that infinitely small quantities are thrown out as nothing. And yet a Cambridge mathematician says, even at the present day, that we cannot so easily answer this objection, because we cannot see how it arises!

This principle was adopted as an axiom, or rather as "a sort of axiom." Now is this really an axiom or otherwise? Is it true or false? Will it make no possible difference in the result whether we throw away as nothing or retain as something these infinitely small quantities? If we subtract one quantity, however small, from another, shall we not at least diminish that other by an amount equal to the quantity subtracted? It seems so to me. Yet Carnot, who has looked so deeply into the "metaphysics" of the calculus, appears, at least occasionally, to entertain a different opinion. For having referred to the brilliant career of the axiom in question, and to the prodigies it had performed in the hands of Leibnitz and others, he adds: "The illustrious author and the celebrated men who adopted his idea (*i. e.*, the above axiom) contented themselves with showing by the solution of problems the most difficult, the fecundity of the principle, and the constant agreement of its results with those of the ordinary analysis, and the ascendency which it gives to the new calculus. These multiplied successes victoriously proved that all the objections were only specious; but these savans did not reply in a direct manner, and the knot of the difficulty remained. There are truths with which all just minds are struck at first, of which, however, the rigorous demonstration escapes for a long time the most skillful." We should not be surprised, however, if we should hereafter find Carnot himself urging the very objection he here pronounces "only specious," and branding the above axiom as an error; for it seems to be one of the established penalties of nature that the man who begins by denying the truth shall end by contradicting himself.

This truth, if it be one, has certainly had to wait a long time on "the skillful" for a demonstration. It is now, indeed, more than two hundred years since it is supposed to have "struck all just minds," and yet, although it has always been objected to, it is just as far as ever from having been demonstrated.

The calculus of Leibnitz, we are told, was "established upon this principle" by its author. If, then, the thing were possible, why did not Leibnitz himself demonstrate this principle, and put the foundation of his system beyond cavil and controversy? Or why did not M. Carnot, or some other admirer of this great fundamental truth, vouchsafe a demonstration of it to the world? Shall it wait for ever on the "most skillful" for a demonstration, and wait in vain? Carnot offers a graceful apology for Leibnitz. "It is not astonishing," says he, "that Leibnitz should not have attempted the rigorous demonstration of a principle which was then generally regarded as an axiom." * But he knew that this was an axiom only with the initiated few, while, on all sides, there came up against it objections from the common sense and reason of mankind. He only replied, if we may believe M. Carnot, by "the solution of the most difficult problems," and by showing "the ascendency which it gave to the new calculus," and thus "victoriously proved that every objection was only specious." "But," adds our author, "he did not reply in a direct manner, and the knot of the difficulty remained." Why, then, did he not reply in a direct manner, and for ever dissipate the knot of the difficulty? The truth is, that the dark knot of the difficulty was in the mind

* Reflexions, etc., Chapter III., p. 14.

of Leibnitz himself, as well as in the minds of those who objected against the logical basis of his method.

If, as Carnot says, Leibnitz failed to reply in a direct manner to the objection in question, it cannot be said that he made no attempt to furnish such a reply. For it is well known that he did attempt such a reply, and also that it was a failure.* "Leibnitz," says M. Comte, "urged to answer, had presented an explanation *entirely erroneous*, saying that he treated infinitely small quantities as *incomparables*, and that he neglected them in comparison with finite quantities, 'like grains of sand in comparison with the sea;' a view which would have completely changed the nature of his analysis by reducing it to a mere approximative calculus," etc.† A greater than M. Comte had, many years before him, said precisely the same thing. "M. Leibnitz," says D'Alembert, "embarrassed by the objections which he felt would be made to infinitely small quantities, such as the differential calculus considered them, has preferred to reduce his infinitely small quantities to be only *incomparables*, which ruined the geometrical exactitude of the calculus." Now, if instead of all this embarrassment, vacillation, and uncertainty, Leibnitz had only demonstrated his fundamental principle, then his reply would have been far more satisfactory. Even the unskillful would have been compelled to recognize its truth, and lay aside their objections to his method. But, as it was, this illustrious man bequeathed, with all its apparent uncertainty and darkness, the fundamental principle of his method to his followers.

* Montucla's Histoire de Mathématiques, Vol. I., Leibnitz.

† Philosophy of Mathematics, Chapter III., p. 99.

"The Marquis de L'Hôpital," one of the most celebrated of those followers, "was the first to write a systematic treatise on the 'Analysis of Infinitely Small Quantities," Hitherto its principles constituted, for the most part, a sort of esoteric doctrine for the initiated; but now, by this most venerable man and accomplished mathematician, they were openly submitted to the inspection of the world. The whole superstructure rests on two assumptions, which the author calls "demands or suppositions." The first of these is thus stated by the Marquis:

I. FIRST DEMAND OR SUPPOSITION.

"We demand that we can take indifferently the one for the other, two quantities which differ from each other only by an infinitely small quantity, or (which is the same thing) that a quantity which is increased or diminished only by another quantity infinitely less than itself, can be considered as remaining the same."* True, it may be considered as remaining the same, if we please; but every intelligent student asks, Will it actually remain the same? If we "increase or diminish" a quantity, ever so little, will it not be increased or diminished? And if we throw out any quantities, however small, as nothing, will not this make some difference in the result? Thus, it seems to be written over the very door of the mathematical school of Leibnitz and de L'Hôpital, "let no man enter here who cannot take his first principles upon trust." When young Bossut, afterward the historian of mathematics, ventured to hint his doubts respecting this first demand, and ask for light: "Never mind," said his

* Analyse des Infiniment Petits, Art. 2, p. 3.

teacher; "go to work with the calculus, and you will soon become a believer."* He took the advice—what else could he do?—and ceased to be a thinker in order to become a worker.

II. SECOND DEMAND OR SUPPOSITION.

"We demand," says the Marquis, "that a curve line can be considered as the assemblage of an infinity of right lines, each infinitely small; or (what is the same thing) as a polygon with an infinite number of sides, each infinitely small, which determine by the angles which they make with each other the curvature of the line."

Now this is the principle which, in the preceding pages, I have so earnestly combated. The truth is, as I shall presently *demonstrate*, that these two false principles or demands lead to errors, which, being opposite and equal, exactly neutralize each other, so that the great inventor of this intellectual machine, as well as those who *worked it* the most successfully, were blindly conducted to accurate conclusions.

In the preface to his work, the Marquis de L'Hôpital says, "The two demands or suppositions which I have made at the commencement of this treatise, and upon which alone it is supported, appear to me so evident, that I believe they can leave no doubt in the mind of attentive readers. I could easily have demonstrated them after the manner of the ancients, if I had not proposed to myself to be short upon the things which are already known, and devote myself principally to

* Most teachers of the present day are wiser: they avoid all such difficulties; they do not state the first principles at all; they just set their pupils to work with the calculus, and they become believers rather than thinkers.

those which are new." I have been curious to know what sort of demonstrations the Marquis had found for this "sort of axioms." It is pretty certain, it seems to me, that they could hardly have been perfectly clear and satisfactory to his own mind, or else he would have laid such demonstrations, like blocks of transparent adamant, at the foundation of his system. It is evident he should have done so, for this would have removed a world of doubt from the opponents of the new system, and a world of difficulty from its friends. Indeed, if there be principles by which his postulates or demands could have been demonstrated, then those principles must have been more evident and satisfactory than these postulates or demands themselves, and should, therefore, have been made to support them. A little time would not, most assuredly, have been misemployed in giving such additional firmness and durability to so vast and complicated and costly a structure.

The third edition of the "Analyse," the one now before me, is "followed by a commentary [nearly half as large as the book itself], for the better understanding of the most difficult places of the work." Now, strange to say, one of these "most difficult places" which a commentary is deemed necessary to clear up, is the first "demand or supposition" which is laid down by the author as self-evident. "The demand, or rather the supposition of article 2, page 3," says the commentary, "which beginners consider only with pain, contains nothing at the bottom which is not very reasonable." Then the commentator proceeds to show, by illustrations drawn from the world of matter, that this first axiom is not unreasonable. Not unreason-

able indeed! Should not the axioms of geometry be reason itself, and so clear in the transcendent light of their own evidence as to repudiate and reject all illustrations from the material world? The very existence of such a commentary is, indeed, a sad commentary on the certainty of the axioms it strives to recommend.

"In fact," says the commentator, "we regard as infinitely exact the operations of geometers and astronomers; they make, however, every day, omissions much more considerable than those of the algebraists. When a geometer, for example, takes the height of a mountain, does he pay attention to the grain of sand which the wind has raised upon its summit? When the astronomers reason about the fixed stars, do they not neglect the diameter of the earth, whose value is about three thousand leagues? When they calculate the eclipse of the moon, do they not regard the earth as a sphere, and consequently pay no attention to the houses, the towns, or the mountains which are found on its surface? But it is much less to neglect only $d\,x$, since it takes an infinite number of $d\,x$'s to make one x; then the differential calculus is the most exact of all calculuses; then the demand of article 2 contains nothing unreasonable. All these comparisons are drawn from the Course of Mathematics of Wolff, tom. 1, p. 418." Thus, from this curious commentary it appears that the editor of the work of de L'Hôpital in 1798, as well as Wolff, the great disciple of Leibnitz, regarded the differential calculus as merely a method of approximation. Leibnitz himself, as we have already seen, was at times more than half inclined to adopt the same view; plainly confessing that he neglected infinitely small quantities in comparison with finite ones, "like

grains of sand in comparison with the sea." Indeed, he must have been forced to this conclusion and fixed in this belief, if pure geometry had not saved him from the error. He certainly expected that the rejection of his infinitesimals would tell on the perfect accuracy of his results; but he found, in fact, that these often coincided exactly with the conclusions of pure geometry, not differing from the truth by even so much as a grain of sand from the sea, or from the solar system itself. But, not comprehending why there should not have been at least an infinitely small error in his conclusions, he simply stood amazed, as thousands have since done, before the mystery of his method, sometimes calling his "infinitely small quantities zeros," sometimes "real quantities," and sometimes "fictions." When he considered these quantities in their origin, and looked at the little lines which their symbols represented, he thought they must be real quantities; but since these quantities might be infinitely less than the imagination of man could conceive, and since the omission of them led to absolutely exact results, he was inclined to believe that they must be veritable zeros. But, not being able to reconcile these opposite views, or to rest in either, he sometimes effected a sort of compromise, and considered his infinitely small quantities as merely analytical "fictions." The great celebrities of the mathematical world since the time of Leibnitz, the most illustrious names, indeed, in the history of the science, may be divided into three classes, and ranged as advocates of these three several views of the differential calculus.

It was long before the true secret was discovered. "After various attempts, more or less imperfect," says

M. Comte, "a distinguished geometer, Carnot, presented at last the true, direct, logical explanation of the method of Leibnitz, by showing it to be founded on the principle of the necessary compensation of errors, this being, in fact, the precise and luminous manifestation of what Leibnitz had vaguely and confusedly perceived."* Now Carnot owed absolutely no part of his discovery to Leibnitz. If Leibnitz, indeed, obscurely perceived the existence of such a compensation of errors in the working of his method, he has certainly nowhere given the most obscure intimation of it in his writings. Such a hint from the master, however unsupported by argument, would have served at least to put some of his followers on the true path of inquiry. But no such hint was given. Leibnitz, it is true, perceived several things very obscurely; but the real secret of his method was not one of them. Hence he put his followers on the wrong scent only, and never upon the true one. Indeed, if he had suspected his system of a secret compensation of errors, then he must also have suspected that the two "demands or suppositions" on which it rested were both false, and it would not have been honest in him to lay them down as self-evident truths or axioms.

The explanation of Carnot is certainly, as far as it goes, perfectly satisfactory. In the second edition of his work he quotes with an evident and justifiable satisfaction, the approbation which the great author of the *Theory of Functions* had bestowed on his explanation. "In terminating," says he, "this exposition of the doctrine of compensations, I believe I may honor myself with the opinion of the great man whose

* Philosophy of Mathematics, Chap. III., p. 100.

recent loss the learned world deplores, *Lagrange!* He thus expresses himself on the subject in the last edition of his 'Théorie des Fonctions Analytiques:'—

"'In regarding a curve,' says Lagrange, 'as a polygon of an infinite number of sides, each infinitely small, and of which the prolongation is the tangent of the curve, it is clear that we make an erroneous supposition; but this error finds itself corrected in the calculus by the omission which is made of infinitely small quantities. This can be easily shown in examples, but it would be, perhaps, difficult to give a general demonstration of it.'

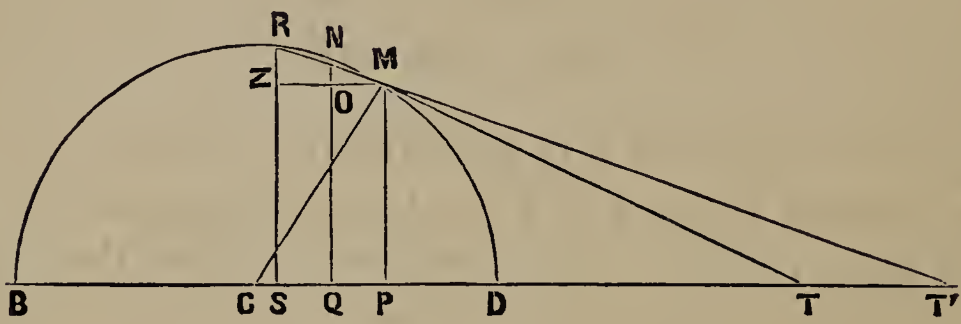
"Behold," exclaims Carnot, "my whole theory resumed with more clearness and precision than I could put into it myself!" * Let us, then, see this theory, or rather its demonstration. Carnot begins with a special case.

"For example," says he, "let it be proposed to

* It is, then, Carnot's emphatic opinion that the two demands or postulates of the method of Leibnitz are both "clearly erroneous suppositions" or false hypotheses. Yet, as we have seen, when the first of these demands was assailed by others as untrue, he pronounces the objection "only specious;" he excuses Leibnitz for not having demonstrated it, because it was "generally regarded as an axiom," and even places it among those truths which, at first, "strike all just minds," but of which the demonstrations long remain to be discovered. Now, how could Leibnitz have demonstrated an error? Or how long will it be before such a thing is demonstrated? Or, again, if any one objects to receiving as true a manifest error, how can it be said that his objection is "only specious?" The truth is, that instead of that metaphysical clearness and firmness of mind which never loses sight of a principle, but carries it as a steady light into all the dark regions of speculation, there is some little wavering and vacillation in the views of Carnot, and occasionally downright contradictions, especially in what he says in regard to the method of Leibnitz.

draw a tangent to the circumference B M D at the given point M.

"Let C be the centre of the circle, D C B the axis;



suppose the abscissa D P $= x$, the ordinate M P $= y$, and let T P be the subtangent required.

"In order to find it, let us consider the circle as a polygon with a very great number of sides; let M N be one of these sides, prolonged even to the axis; that will evidently be the tangent in question, since that line does not penetrate to the interior of the polygon; let fall the perpendicular M O upon N Q, which is parallel to M P, and name a the radius of the circle; this supposed, we shall evidently have

$$\mathrm{MO} : \mathrm{NO} :: \mathrm{TP} : \mathrm{MP},$$

or

$$\frac{\mathrm{MO}}{\mathrm{NO}} = \frac{\mathrm{TP}}{\mathrm{MP}} = \frac{\mathrm{TP}}{y}.$$

On the other hand, the equation of the curve for the point M being $y^2 = 2ax - x^2$, it will be for the point N, $(y + \mathrm{NO})^2 = 2a(x + \mathrm{MO}) - (x + \mathrm{MO})^2$, taking from this equation the first, found for the point M, and reducing, we have

$$\frac{\mathrm{MO}}{\mathrm{NO}} = \frac{2y + \mathrm{NO}}{2a - 2x - \mathrm{MO}};$$

equaling this value of $\frac{MO}{NO}$ to that which has been found above, and multiplying by y, it becomes

$$TP = \frac{y(2y + NO)}{2a - 2x - MO}.$$

"If then M O and N O were known, we should have the required value of T P; but these quantities M O N O are very small, since they are less than the side M N, which, by hypothesis, is itself very small. Then we can neglect without sensible error these quantities in comparison with the quantities $2y$ and $2x - 2a$ to which they are added. Then the equation reduces itself to

$$TP = \frac{y^2}{a - x},$$

which it was necessary to find.

"If this result is not absolutely exact, it is at least evident that in practice it can pass for such, since the quantities M O, N O are extremely small; but any one who should have no idea of the doctrine of the infinite, would perhaps be greatly astonished if we should say to him that the equation $TP = \frac{y^2}{a - x}$, not only approaches the truth very nearly, but is really most perfectly exact; it is, however, a thing of which it is easy to assure one's self by seeking T P, according to the principle that the tangent is perpendicular to the extremity of the radius; for it is obvious that the similar triangles C P M, M P T, give

$$CP : MP :: MP : TP,$$

or $$a - x : y :: y : \mathrm{T\,P};$$

hence, $$\mathrm{T\,P} = \frac{y^2}{a - x}, \text{ as above.}$$

"Let us see, then, how in the equation

$$\mathrm{T\,P} = \frac{y\,(2\,y + \mathrm{N\,O})}{2\,a - 2\,x - \mathrm{M\,O}},$$

found above, it has happened that in neglecting M O and N O we have not altered the justness of the result, or rather how that result has become exact by the suppression of these quantities, and why it was not so before.

"But we can render very simply the reason why this has happened in the solution of the problem above treated, in remarking that, the hypothesis from which it set out being false, *since it is absolutely impossible that a circle can ever be considered as a true polygon*, whatever may be the number of its sides, there ought to result from this hypothesis an error in the equation

$$\mathrm{T\,P} = \frac{y\,(2\,y + \mathrm{N\,O})}{2\,a - 2\,x - \mathrm{M\,O}},$$

and that the result $\mathrm{T\,P} = \dfrac{y^2}{a - x}$ being nevertheless certainly exact, as we prove by the comparison of the two triangles C P M, M P T, we have been able to neglect M O and N O in the first equation; and indeed we ought to have done so in order to rectify the calculus, and to destroy the error which had arisen from the false hypotheses from which we had set out. To neglect the quantities of that nature is then not

only permitted in such a case, but it is necessary: it is the sole manner of expressing exactly the conditions of the problem." *

"The exact result $TP = \frac{y^2}{a - x}$ has then been obtained only by a compensation of errors; and that compensation can be rendered still more sensible by treating the above example in a little different manner, that is to say, by considering the circle as a true curve, and not as a polygon.

"For this purpose, from a point R, taken arbitrarily at any distance from the point M, let the line R S be drawn parallel to M P, and through the points R and M let the secant R T′ be drawn; we shall evidently have

$$T'P : MP :: MZ : RZ,$$

and dividing T′ P into its parts, we have

$$TP + TT' = MP\frac{MZ}{RZ}.$$

This laid down, if we imagine that R S moves parallel to itself in approaching continually to M P, it is obvious that the point T′ will at the same time approach more and more to the point T, and that we can consequently render T′ T as small as we please without the established relations ceasing to exist. If then I neglect the quantity T′ T in the equation I have just found, there will in truth result an error in the equation $TP = MP\frac{MZ}{RZ}$, to which the first will then be

* This last expression seems a little obscure, since it is difficult to perceive how the neglect of such quantities is necessary "to express the conditions of the problem."

reduced; but that error can be attenuated as much as we please by making R S approach M P as much as will be necessary; that is to say, that the two members of that equation may be made to differ as little as we please from equality.

"In like manner we have $\frac{\text{M Z}}{\text{R Z}} = \frac{2y + \text{R Z}}{2a - 2x - \text{M Z}}$, and this equation is perfectly exact, whatever may be the position of R; that is to say, whatever may be the values of M Z and R Z. But the more R S shall approach M P, the more small will the lines M Z and R Z become, and if we neglect them in the second member of the equation, the error that will result therefrom in the $\frac{\text{M Z}}{\text{R Z}} = \frac{y}{a - x}$ to which it will then be reduced, would, as in the first, be rendered as small as we might think proper.

"This being so, without having regard to the errors which I may always render as small as I please, I treat the two equations

$$\text{T P} = \text{M P}\,\frac{\text{M Z}}{\text{R Z}} \text{ and } \frac{\text{M Z}}{\text{R Z}} = \frac{y}{a - x},$$

which I have just found, as if they were both perfectly exact; substituting then in the first, the value of $\frac{\text{M Z}}{\text{R Z}}$ taken from the last, I have for the result

$$\text{T P} = \frac{y^2}{a - x}, \text{ as above.}$$

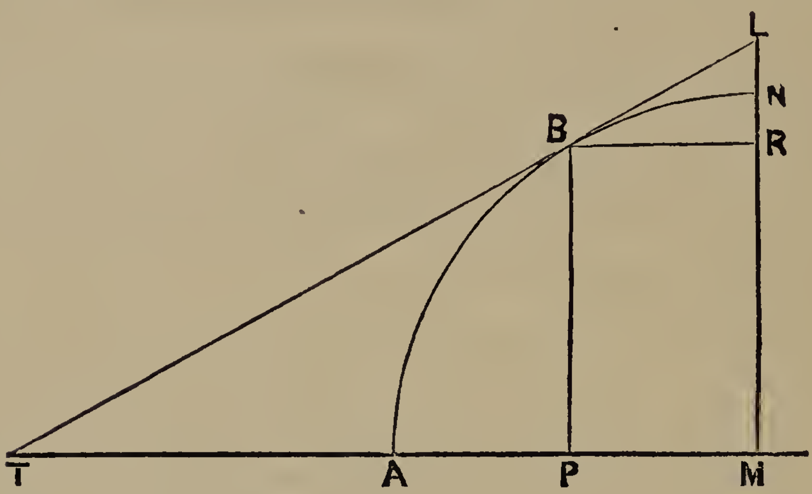
This result is perfectly just, since it agrees with that which we obtain by comparing the triangles C P M,

M P T, and yet the equations $TP = y\,\frac{MZ}{RZ}$ and $\frac{MZ}{RZ} = \frac{y}{a - x}$, from which it is deduced, are both certainly false, since the distance of R S from M P has not been supposed nothing, nor even very small, but equal to any arbitrary line whatever. It follows, as a necessary consequence, that the errors have been mutually compensated by the combination of the two erroneous equations."

"Behold, then," says Carnot, "the fact of the compensation of errors clearly and conclusively proved." He very justly concludes that there is a mutual compensation of errors in the case considered by him, because the combination of the two imperfect equations resulted in an absolutely perfect one. If, however, he had pointed out the error on the one side and on the other, and then proved that they were exactly equal and opposite, his exposition would, it seems to me, have been rather more "precise and luminous." This is precisely the course pursued by Bishop Berkeley in his demonstration of the same fact. It may be well, therefore, to give his illustrative proof of this compensation of errors in the ordinary use of the calculus. "Forasmuch," says he, "as it may perhaps seem an unaccountable paradox that mathematicians should deduce true propositions from false principles, be right in the conclusion, and yet err in the premises, I shall endeavor particularly to explain why this may come to pass, and *show how error may bring forth truth, though it cannot bring forth science.*"

"In order, therefore, to clear up this point, we will suppose, for instance, that a tangent is to be drawn to

a parabola, and examine the progress of the affair as it is performed by infinitesimal differences. Let A B be a curve, the abscissa A P $= x$, the ordinate P B $= y$; the difference of the abscissa P M $= d\,x$, the difference



of the ordinate R N $= d\,y$. Now, by supposing the curve to be a polygon, and consequently B N, the increment or difference of the curve to be a straight line coincident with the tangent, and the differential triangle B R N to be similar to the triangle T P B, the subtangent is found a fourth proportional to R N : R B : P B; that is, to $d\,y : d\,x : y$. Hence the subtangent will be $\frac{y\,d\,x}{d\,y}$. But then there is an error arising from the forementioned false supposition (*i. e.*, that the curve is a polygon with a great number of sides), whence the value of P T comes out greater than the truth; for in reality it is not the triangle R N B, but R L B, which is similar to P B T, and therefore (instead of R N) R L should have been the fourth term of the proportion; *i. e.*, R N + N L, *i. e.*, $d\,y + z$; whence the true expression for the subtangent should have been $\frac{y\,d\,x}{d\,y + z}$. There was, therefore, an error of

defect in making dy the divisor, which error is equal to z; *i. e.*, N L the line comprehended between the curve and the tangent. Now, by the nature of the curve $y^2 = px$, supposing p to be the parameter, whence by the rule of differences, $2y\,dy = p\,dx$ and $dy = \frac{p\,dx}{2y}$. But if you multiply $y + dy$ by itself, and retain the whole product without rejecting the square of the difference, it will then come out, by substituting the augmented quantities in the equation of the curve, that $dy = \frac{p\,dx}{2y} - \frac{dy^2}{2y}$ truly. There was, therefore, an error of excess in making $dy = \frac{p\,dx}{dy}$, which followed from the erroneous rule of differences. And the measure of this error is $\frac{dy^2}{2y} = z$. Therefore the two errors, being equal and contrary, destroy each other, the first error of defect being corrected by a second of excess.

"If you had committed only one error, you would not then come at a true solution of the problem. But by virtue of a twofold mistake you arrive, though not at science, yet at truth. For science it cannot be called when you proceed blindfold and arrive at truth not knowing how or by what means. To demonstrate that z is equal to $\frac{dy^2}{2y}$, let B R or dx be m, and R N or dy be n. By the thirty-third proposition* of the first book of the Conics of Apollonius, and from similar triangles,

* Which is, that the subtangent T P is equal to $2x$.

$$2x : y :: m : n + z = \frac{my}{2x}.$$

Likewise from the nature of the parabola $y^2 + 2yn + n^2 = px + pm$, and $2yn + n^2 = pm$; wherefore $\frac{2yn + n^2}{p} = m$; and because $y^2 = px$, $\frac{y^2}{p}$ will be equal to x. Therefore, substituting these values instead of m and x, we shall have

$$n + z = \frac{my}{2x} = \frac{2y^2n + yn^2}{2y^2};$$

that is, $$n + z = \frac{2yn + n^2}{2y},$$

which being reduced gives

$$z = \frac{n^2}{2y} = \frac{dy^2}{2y}. \qquad \text{Q. E. D.*}$$

Thus it is shown that when we seek the value of the subtangent on the supposition that the curve is a polygon, we make dy too small by the line N L. On the other hand, it is shown that when in seeking the value of dy from the differential equation of the curve, we throw out the minus quantity $\frac{dy^2}{2y}$ as infinitely small in comparison with dy, we make dy too great by this quantity $\frac{dy^2}{2y}$. But if we first make dy too small by N L, and then too great by $\frac{dy^2}{2y}$, it only remains to be shown that these two quantities are ex-

* The Analyst, XXI. and XXII. Berkeley's Works, Vol. II., p. 422.

actly equal in order to establish the fact of a compensation of errors. Accordingly, this is done by the Bishop of Cloyne, with the addition of the "Q. E. D." That is to say, the error resulting from one "demand or supposition" of the Leibnitzian method is corrected by the error arising from its other demand or supposition.

It is not true, then, as M. Comte alleges, that Carnot was the first to present the true "explanation of the method of Leibnitz." This honor is due to the Bishop of Cloyne, not to the great French minister of war; to the philosopher, not to the mathematician; for the explanation of Berkeley preeeded that of Carnot by more than half a century. Both explanations rest, as we have seen, on particular examples instead of general demonstrations. Hence Lagrange, after approving the explanation or adopting it as his own view of the subject, adds: "This may be easily shown in examples, but it would be, perhaps, difficult to give a general demonstration." Carnot dissents. "I believe," says he, that "in the demonstration which I have given of it" there "is wanting nothing either of exactitude or of generality." His *general* demonstration, however, is metaphysical rather than mathematical—a sort of demonstration which does not always carry irresistible conviction to the mind.

Having exhibited his examples, Carnot proceeds to ascertain "the sign by which it is known that the compensation has taken place in operations similar to the preceding, and the means of proving it in each particular case." This is done only by a process of "general reasoning," as it is very properly called by M. Comte, and it is fairly exhibited in his *Philosophy*

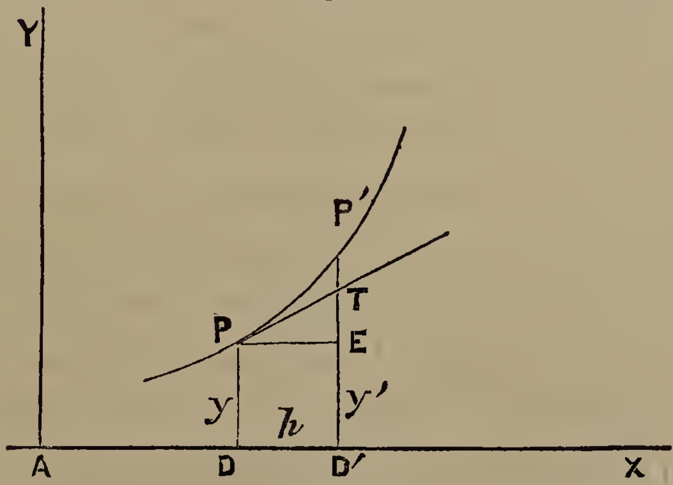
of Mathematics. "In establishing," says M. Comte, "the differential equation of phenomena, we substitute for the immediate elements of the different quantities considered other simpler infinitesimals, which differ from them infinitely little in comparison with them, and this substitution constitutes the principal artifice of the method of Leibnitz, which without it would possess no real facility for the formation of equations. Carnot regards such an hypothesis as really producing an error in the equation thus obtained, and which for this reason he calls *imperfect;* only it is clear that this error must be infinitely small. Now, on the other hand, all the analytical operations, whether of differentiation or of integration, which are performed upon these differential equations in order to raise them to finite equations by eliminating all the infinitesimals which have been introduced as auxiliaries, produce as constantly by their nature, as is easily seen, other analogous errors, so that an exact compensation takes place, and the finite equations, in the words of Carnot, become *perfect.* Carnot views, as a certain and invariable indication of the actual establishment of this necessary compensation, the complete elimination of the various infinitely small quantities, which is always, in fact, the final object of all the operations of the transcendental analysis; for if we have committed no other infractions of the general rules of reasoning than those thus exacted by the very nature of the infinitesimal method, the infinitely small errors thus produced cannot have engendered other than infinitely small ones in all the equations, and the relations are necessarily of a rigorous exactitude as soon as they exist between finite quantities alone, since the

only errors then possible must be finite ones, while none such can have entered. All this general reasoning is founded on the conception of infinitesimal quantities, regarded as indefinitely decreasing, while those from which they are derived are regarded as fixed."*

Lagrange had, perhaps, no objection to offer to this "general reasoning;" it appears certain that he did not regard it as a "general demonstration." "It would, perhaps," says he, "be difficult to give a general *demonstration*" of the fact of a compensation of errors in the use of the calculus. It is easy to give a far more general demonstration than that proposed by either Carnot or Berkeley. For the one of these relates, as we have seen, merely to the question of finding a tangent to the circumference of a circle at a given point,† and the other to the same problem with reference to the parabola. Now, this compensation of errors may be demonstrated to take place in the process for finding the tangent to curve lines in general.

Let $y = \mathrm{F}\,x$ be the equation of such a curve, in

Fig. 1.



* Philosophy of Mathematics, Book I., Chap. III., p. 101.

† Carnot, I am aware, has furnished a second example; but this does not make the proof general.

which y is equal, not to one particular function of x, as in the case of the circle or parabola, but to any algebraic function whatever. Then, if the curve be convex toward the axis of x, as in Fig. 1, and if an increment h be given to the abscissa $A D = x$, the ordinate y will take an increment $E P'$, whose value may thus be found:

$$P D \text{ or } y = F x,$$

$$P' D' \text{ or } y' = F(x+h) = F x + \frac{dy}{dx}\frac{h}{1} + \frac{d^2 y}{dx^2}\frac{h^2}{1.2}$$

$$+ \frac{d^3 y}{dx^3} \cdot \frac{h^3}{1.2.3} +, \text{ etc.},$$

by Taylor's Theorem,

or $$P' D' - P D, \text{ or } E P' = \frac{dy}{dx} \cdot \frac{h}{1} + \frac{d^2 y}{dx^2}\frac{h^3}{1.2}$$

$$+ \frac{d^3}{dx^3}\frac{h^3}{1.2.3} + \text{ etc.}, (= M);$$

and the ordinate of the tangent line $P T$ will take the corresponding increment $E T$, whose value, found in the same way, gives

$$E T = \frac{dy}{dx}\frac{h}{1}.$$

Hence

$$E P' - E T = P' T = \frac{d^2 y}{dx^2}\frac{h^2}{1.2} + \frac{d^3 y}{dx^3} \cdot \frac{h^3}{1.2.3} +, \text{ etc.}$$

Now, the subtangent s is the fourth term of the exactly true proportion,

$$E T : h :: y : s;$$

but $E T$, being unknown, cannot be used for the pur-

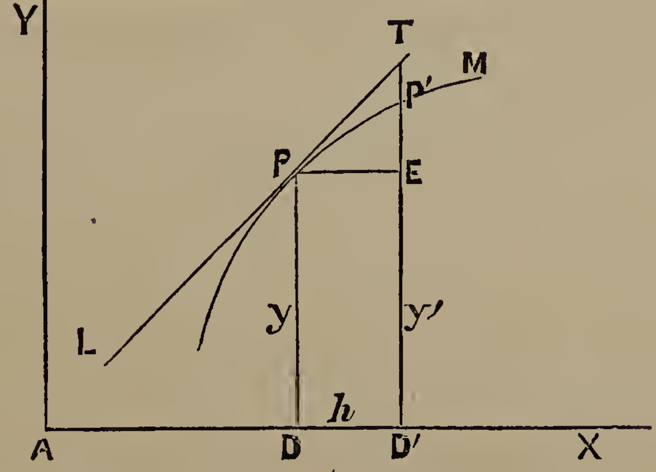
pose of finding the value of s. Hence E P′ is in the method of Leibnitz substituted for E T, and this substitution is justified on the ground that the difference between the two quantities is so very small. But still this difference is, as we have seen,

$$P'T = \frac{d^2 y}{d x^2} \frac{h^2}{1 . 2} + \frac{d^3 y}{d x^3} \frac{h^3}{1 . 2 . 3}, \text{ etc.}$$

When the operator comes, however, to find the value of P′ E from the equation of the curve, this value of P′ T is precisely the quantity thrown away as nothing by the side of an infinitely small one of the first order. Thus, by the one step, the true value is made too great by the quantity P′ T, and by the other the substituted value is reduced by precisely the same amount P′ T. That is to say, the same quantity was first added and then subtracted, which, of course, made no difference in the result.

If the curve be concave toward the axis of x, as

Fig. 2.



seen in Fig. 2, then the true value, T E, or the line which gives the exact value of s by the proportion,

$$TE : h : : y : s,$$

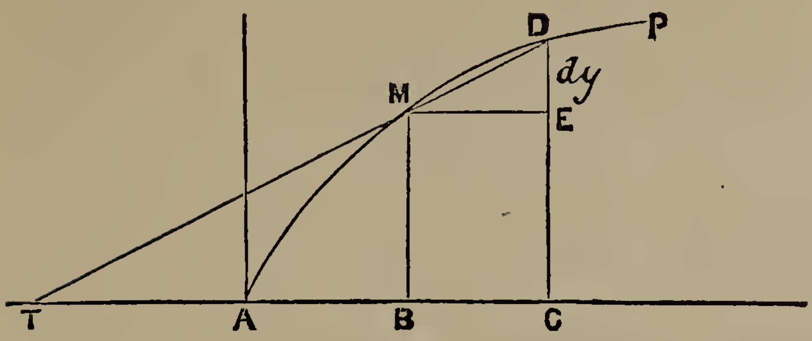
will be made too small by the substitution of P′ E in its place. But, in this case, the value of P′ T, or

$$\frac{d^2 y}{d x^2} \frac{h^3}{1 \, . \, 2} + \frac{d^3 y}{d x^3} \frac{h^3}{1 \, . \, 2 \, . \, 3} +, \text{etc.},$$

which is rejected in finding the value of P′ E, is a negative quantity; and, consequently, in throwing it away from the value of P′ E as nothing, that value was increased by the same amount it had been diminished. That value of P′ T is negative, because the curve being concave toward the axis of x, its first term $\frac{d^2 y}{d x^2} \cdot \frac{h}{1 \, . \, 2}$ is negative, and, since it is supposed very small, this term is greater than the sum of all the others.* In this case, then, the same quantity was subtracted and added, which, of course, did not affect the result. Behold the very simple process which, by means of signs and symbols and false hypotheses, has been transformed into the sublime mystery of the transcendental analysis.

In spite of its logical defects, however, the method of Leibnitz has generally been adopted in practice; because of the facility with which it reduces questions of the infinitesimal analysis to equations, and arrives at their solutions. Suppose, for example, the question be to find the tangent line to the point M of any curve A P, which is given by its equation. The method of Leibnitz identifies the infinitely small chord M D with the corresponding arc of the curve, and, consequently, regards the figure M D E, composed of the small arc M D and the increments of x and y, as a rectilinear

* See any work on the Differential Calculus.



$BC = dx$
$DE = dy$
$AB = x$
$BM = y$

triangle. (This is, in fact, the differential triangle of Barrow.) Hence, according to this method,

$$\frac{DE}{ME} \text{ or } \frac{dy}{dx} = \frac{BM}{TB},$$

or the tangent of the angle which the tangent line required makes with the axis of x. To find the value of this tangent, then, it is only necessary to ascertain the value of $\frac{dy}{dx}$ for the point M from the equation of the curve.

If the curve, for example, be the common parabola, whose equation is $y^2 = 2px$, the value in question may be easily ascertained. Thus, give to A B, or to x for the point M, the infinitely small increment dx, and B M, or y for the same point, will take the infinitely small increment dy. Then,

$$(y + dy)^2 = 2p(x + dx),$$

or $$y^2 + 2y\,dy + dy^2 = 2px + 2p\,dx.$$

Hence $$2y\,dy + dy^2 = 2p\,dx.$$

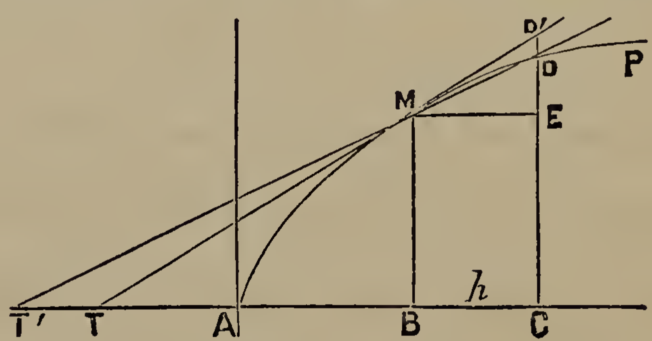
But dy^2, being an infinitely small quantity of the second order, may be rejected as nothing by the side of $2y\,dy$, and hence we have

$$2\,y\,d\,y = 2\,p\,d\,x$$

$$\frac{d\,y}{d\,x} = \frac{p}{y},$$

for the value of the tangent required, which, by the rigorous method of geometry, is known to be perfectly exact.

Now, as we have already seen, there are in this process two errors which correct each other; the one arising from the false hypothesis that the curve A P is made up of infinitely small right lines, such as M D; and the other from the equally false postulate or demand that the infinitely small quantity $d\,y^2$ may be rigorously regarded as nothing by the side of $2\,y\,d\,y$. These false hypotheses are, however, wholly unnecessary, and only serve to darken science by words without knowledge. That is to say, we may, in the perfectly clear light of correct principles, do precisely the same thing that is done in the method of Leibnitz by means of his false hypotheses and false logic. To show this, let us resume the question of finding the tangent to point M of the common parabola A P. The



tangent line T D′ has, according to the definition, only the point M in common with the curve, and

$$\frac{\mathrm{D}'\,\mathrm{E}}{\mathrm{M}\,\mathrm{E}} = \frac{\mathrm{B}\,\mathrm{M}}{\mathrm{T}\,\mathrm{B}}.$$

But D′ E is unknown, since the equation of the tangent line, the very thing to be determined, is not given. Hence we adopt or apply the method of Leibnitz, without adopting his view of that method. That is to say, we start with the expression $\frac{\mathrm{D\,E}}{\mathrm{M\,E}}$ instead of $\frac{\mathrm{D'\,E}}{\mathrm{M\,E}}$, or we take the small quantity D E as the same with D′ E, just as he does; but not because D E is the same as D′ E, or because M D coincides with M D′. On the contrary, we set out with $\frac{\mathrm{D\,E}}{\mathrm{M\,E}}$ because its value may be found from the equation of its curve, and because its limit is $\frac{\mathrm{D'\,E}}{\mathrm{M\,E}}$, the thing to be determined. Thus, give any increment B C or h to x, and y will take a corresponding increment D E or k. Then

$$(y + k)^2 = 2p\,(x + h)$$

$$\frac{k}{h} = \frac{p}{y} - \frac{k^2}{2y}.$$

Now, if we conceive h to become less and less, then will k also decrease; but the ratio $\frac{k}{h}$ will continually increase, and approach more and more nearly to an equality with $\frac{\mathrm{D'\,E}}{\mathrm{M\,E}}$, which is a constant quantity. It is obvious that by making h continue to decrease, the fraction $\frac{\mathrm{D\,E}}{\mathrm{M\,E}}$ may be made to differ as little as we

please from $\frac{\text{D}'\,\text{E}}{\text{M E}}$, and hence $\frac{\text{D}'\,\text{E}}{\text{M E}}$ is the limit of $\frac{\text{D E}}{\text{M E}}$ or $\frac{k}{h}$. But since the two members of the above equation are always equal, their limits are equal. That is to say, the limit of

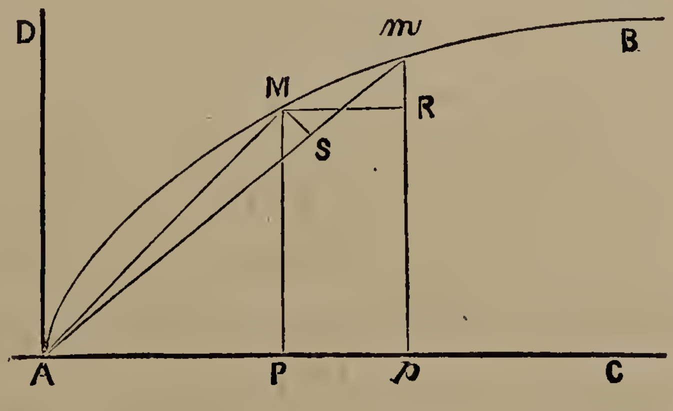
$$\frac{k}{h} \text{ is } \frac{\text{D}'\,\text{E}}{\text{M E}}, \text{ or } \frac{dy}{dx} = \frac{p}{y},$$

as above found. Thus, the two processes not only lead to the same result, but they are, from beginning to end, precisely the same. The steps in both methods are precisely the same, and the only difference is in the rationale or explanation of these steps. In the one method the steps are only so many false hypotheses or assertions, while, in the other, they are carried on in the light of clearly correct principles. In the method of limits we begin with $\frac{\text{D E}}{\text{M E}}$ in order to find the value of $\frac{\text{D}'\,\text{E}}{\text{M E}}$, not because the two lines D E and D′ E are equal to each other, or because the difference between them is so small that it is no difference at all, but simply because the limit of $\frac{\text{D E}}{\text{M E}}$, which may be easily found from the equation of the curve, is exactly equal to the constant quantity $\frac{\text{D}'\,\text{E}}{\text{M E}}$, which is the quantity required.

The principle of this case is universal in its application. That is to say, in the method of limits we may always put one set of variables for another, pro-

vided that in passing to the limit the result will be the same. We may not only do this, but in many cases we must do it, in order to arrive at the desired result. It is, in fact, the sum and substance of the infinitesimal analysis to put one set of quantities for another; *i. e.*, of auxiliary quantities for the quantities proposed to be found, in order to arrive indirectly at the result or value, or relation, which cannot be directly obtained. In the method of Leibnitz, it is supposed that one set of quantities may be put for another, because they differ so little from each other that they may be regarded as rigorously equal, and that an infinitely small quantity may be rejected as "absolutely nothing." On the other hand, the method of limits proceeds on the principle that any one quantity may be put for another, provided that in passing to their limits, *the limit of their difference is zero.*

In order to illustrate his first "demand or supposition," the Marquis de L'Hôpital says: "We demand, for example, that we can take A p for A P, p m for



P M, the space A p m for the space A P M, the little space M P p m for the little rectangle M P p R, the little sector A M m for the little triangle A M S, the angle p A m for the angle P A M." Now he sup-

posed that we can with impunity take the one of these several quantities for the other, because they are equal, and also that an infinitely small quantity may be rejected as nothing with equal impunity. But, in fact, these quantities can, in the infinitesimal analysis, be respectively taken for one another, because their limits are precisely the same, and because, by throwing out the indefinitely small increments as nothing, or by making them zeros, we pass to their limits, which are the same. The space M P p m, for example, is not equal to the space M P p R, but always differs from it by the little space M R m. But yet M P p R may be put for M P p m, because their limit is precisely the same line M P, and because when P p is made equal to zero, or treated as nothing, the limit M P is reached.

"We demand," says the Marquis de L'Hôpital, "that we can take indifferently the one of two quantities for the other which differ from each other only by a quantity infinitely small; or (what is the same thing) that a quantity which is augmented or diminished by another quantity infinitely small can be considered as remaining the same." This demand is refused. The two quantities are not equal; they differ by an indefinitely small quantity, but their limits are the same; and when the indefinitely small difference is reduced to nothing, the same limit or value is obtained. Leibnitz put the one of two quantities for another, because they were the same, whereas he should have done so because their limits were the same. Again, in throwing out indefinitely small quantities as zeros, he supposed that, instead of affecting the result by this step, everything would "remain the same;" whereas,

in fact, this step perfected the operation and reached the true result by passing to the limit. Thus, the true route of the infinitesimal analysis is an indirect one, and Leibnitz, by seeking to make it direct, only caused it to appear absurd.

CHAPTER VII.

THE METHOD OF NEWTON.

THE method of Newton, as delivered by himself, has never been free from difficulties and objections. Indeed, even among learned mathematicians and his greatest admirers there have been obstinate disputes respecting his explanation or view of his own method of "prime and ultimate ratios." The very first demonstration, in fact, of the first book of his *Principia*, in which he lays the corner-stone of his whole method, has long been the subject of controversy among the friends and admirers of the system; each party showing its veneration for the great author by imputing its own views to him, and complaining of the misunderstanding and wrong interpretation of the other. This controversy has, no doubt, been of real service to the cause of science, since it enables the studious disciple of Newton to obtain a clearer insight into the principles and mechanism of his method than he himself ever possessed. It has, indeed, been chiefly by the means of this controversy that time and the progress of ideas have cleared away the obscurities which originally hung around the great invention of Newton. But if we would profit by the labors of time in this respect, as well as by those of Sir Isaac, we must lay aside the spirit in which the controversy has been carried on, and view all sides and all pretended demon-

strations with an equal eye, not even excepting those of the *Principia* itself.

The corner-stone or foundation of Newton's method is thus laid in the *Principia: "Quantities, and the ratios of quantities, that during any finite time constantly approach each other, and before the end of that time approach nearer than any given difference, are ultimately equal."*

"If you deny it, suppose them to be ultimately unequal, and let D be their ultimate difference. Therefore they cannot approach nearer to equality than by that given difference D, which is against the supposition." *

The above demonstration is thus given by Dr. Whewell, "Prop. I. (Newton, Lemma I.):

"Two quantities which constantly tend towards equality while the hypothesis approaches its ultimate form, and of which the difference, in the course of approach, becomes less than any finite magnitude, are ultimately equal."

"The two quantities must either be ultimately equal, or else ultimately differ by a finite magnitude. If they are not ultimately equal, let them ultimately have for their difference the finite magnitude D. But by supposition, as the hypothesis approaches its ultimate form, the differences of the two magnitudes become less than any finite magnitude, and therefore less than the finite magnitude D. Therefore D is not the ultimate difference of the quantities. Therefore they are not ultimately unequal. Therefore they are ultimately equal." †

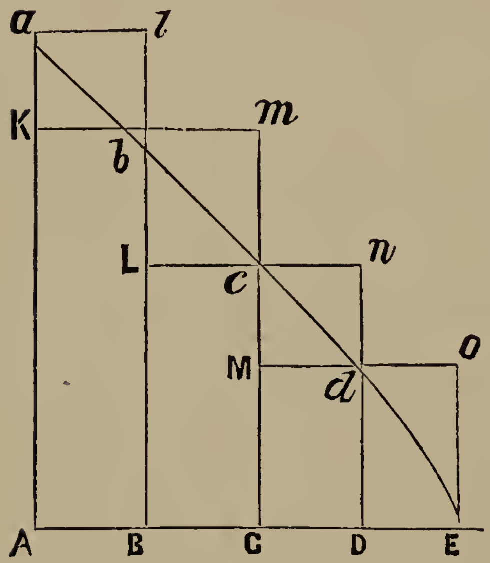
* Principia, Book I., Section I., Lemma I.

† Doctrine of Limits, Book II.

In the two following lemmas Newton proceeds to give particular instances or illustrations of the import of the above general proposition. As these are necessary to render his meaning plain, they are here added:

LEMMA II.

"*If in any figure* A a c E, *terminated by the right lines* A a, A E, *and the curve* a c E, *there be inscribed any number of parallelograms,* A b, B c, C d, etc., *comprehended under equal bases* A B, B C, C D, etc., *and the sides* B b, C c, D d, etc., *parallel to one side* A a *of the figure, and the parallelograms* a K b l, b L c m, c M d n, etc., *are completed. Then if the breadth of those parallelograms be supposed to be diminished, and their number to be augmented* in infinitum; *I say that the ultimate ratios which the inscribed figure* A K b L c M d D, *the circumscribed figure* A a l b m c n d o E, *and curvilinear figure* A a b c d E, *will have to one another, are ratios of equality.*



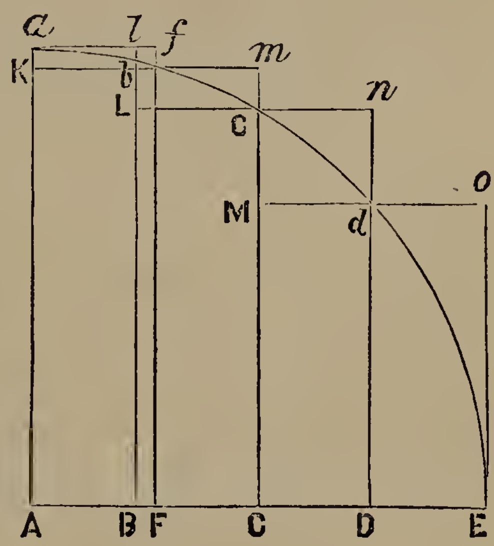
"For the difference of the inscribed and circumscribed figures is the sum of the parallelograms K l, L m, M n, D o, that is (from the equality of all their bases), the rectangle under one of their bases K b, and the sum of their altitudes A a, that is, the rectangle A B l a. But this rectangle, because its breadth A B is supposed diminished *in infinitum*, becomes less than any given space. And therefore (by Lem. I.) the figure

inscribed and circumscribed become ultimately equal one to the other, and much more will the intermediate curvilinear figure be ultimately equal to either. Q. E. D."

LEMMA III.

"*The same ultimate ratios are also ratios of equality, when the breadths* A B, B C, D C, etc., *of the parallelograms are unequal, and are all diminished* in infinitum.

"For suppose A F equal to the greatest breadth, and complete the parallelogram F A a f. This parallelogram will be greater than the difference of the inscribed and circumscribed figures; but, because its breadth A F is diminished *in infinitum*, it will become less than any given rectangle. Q. E. D.



"COR. 1. Hence the ultimate sum of those evanescent parallelograms will in all parts coincide with the curvilinear figure.

"COR. 2. Much more will the rectilinear figure comprehended under the chords of the evanescent arcs $a\,b$, $b\,c$, $c\,d$, etc., ultimately coincide with the curvilinear figure.

"COR. 3. And also the circumscribed rectilinear figure comprehended under the tangents of the same arcs.

"COR. 4. And therefore these ultimate figures (as to their perimeters $a\,c$ E), are not rectilinear, but curvilinear limits of rectilinear figures."

In these celebrated demonstrations, as well as in those which follow, there are very great obscurities and difficulties. The objections to them appear absolutely insuperable. How, for example, can the circumscribed figures in lemmas two and three ever become equal to the curvilinear space A *a* E? If these spaces should ever become equal, then the line A *l b m c n d o* E would necessarily coincide with the curve A *b c d* E, which seems utterly impossible, since a broken line whose sides always make right angles with each other cannot coincide with a curve line. I should not, indeed, believe that the author of the *Principia* contemplated such a coincidence, if his express words, as well as the validity of his demonstration, did not require me to believe it; that is, if he had not expressly said in the first corollary, that "the ultimate sum of these evanescent parallelograms will in all parts *coincide* with the curvilinear figure." The supposition of such a coincidence, even if it were conceivable, leads to an absurdity. For the sum of the horizontal lines *a l*, *b m*, *c n*, *d o*, or however far their number may be augmented, will always be equal to the line A E, and the sum of the corresponding vertical lines *l b*, *m c*, *n d*, *o* E, etc., will always be equal to the line A *a*. Hence, if the two figures should ultimately coincide, then the line A *l b m c n d o* E, or its equal A *b c d* E, would be equal to the sum of the two lines, A E and A *a*. Or, if the curvilinear space A *a* E were the quadrant of a circle, then one-fourth of its circumference would be equal to the sum of the two radii A E and A *a*, or to the diameter, which is impossible. Or, again, if the line *a b c d* E were a straight line, it might be proved by the same reasoning that

the hypothenuse of the right-angled triangle A a E is equal to the sum of its other two sides, which is a manifest absurdity.

The truth is, that the sum of the circumscribed or of the inscribed parallelograms will never become equal to the curvilinear figure. No possible increase of the number of parallelograms can ever reduce their sum to an equality with the curvilinear space. What, then, shall we say to the above demonstrations? Or rather to the demonstration of the first lemma, on which all the others depend? I do not know that any one has ever directly assailed this demonstration; but, unless I am very grievously mistaken, its inherent fallacy may be rendered perfectly obvious. It may be refuted, not only by the *reductio ad absurdum*, or by showing the false conclusions to which it necessarily leads, but by pointing out the inherent defect of its logic.

The attempt is made to prove that the sum of the circumscribed parallelograms will ultimately become equal to the sum of the inscribed parallelograms. Now it is evident that the difference of these sums is a variable quantity which may be made as small as we please. This is, indeed, one of the suppositions of the case; the circumscribed and the inscribed figures are supposed to vary continually, and to "approach nearer the one to the other than by any given difference." Of course, then, they can by this hypothesis be made to "approach nearer to equality than by the given difference D." If you deny the two variable figures to be ultimately equal, says the demonstrator, "suppose them to be ultimately unequal, or let D be their ultimate difference. Therefore they cannot ap-

proach nearer to equality than by that difference D, which is against the supposition." True. If the difference be supposed to be variable, and then supposed to be constant, the one supposition will, of course, be against the other. If the difference in question be a variable, which may be rendered less than any given difference, then, of course, it may be rendered less than the constant quantity D. Hence, to suppose its ultimate value equal to D, is to contradict the first supposition or hypothesis. Indeed, according to that hypothesis, the difference in question has no ultimate or fixed value whatever. It is, on the contrary, always a variable, and its limit is not D, nor any other magnitude but zero. To say, then, that its ultimate value is equal to the constant quantity D, is clearly to contradict the supposition that it is always a variable which may be made to approach as near as we please to zero. But is not that a very precarious and unsatisfactory sort of demonstration which sets out with two contradictory suppositions, and then concludes by showing that the one supposition contradicts the other?

Let us apply this sort of demonstration to another case. If a quantity be reduced, by repeated operations, to one-half of its former value, its successive values may be represented by 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, and so on, *ad infinitum.* By repeating the process sufficiently far, it may be made less than any given quantity, or it may be made to approach as near as we please to zero. But will it ever become zero or nothing? Is the half of something, no matter how small, ever exactly equal to nothing? No one will answer this question in the affirmative. And yet, if the above reasoning be cor-

rect, it may be demonstrated that a quantity may be divided until its half becomes equal to nothing. For, by repeating the process *ad libitum*, it may be supposed to "approach nearer to zero than by a given difference." Hence it will ultimately become equal to zero. "If you deny it, suppose it be ultimately unequal [to zero], and let D be its ultimate difference [from zero]. Therefore it cannot approach nearer to equality [with zero] than by the given difference D, which is against the supposition." But if it be not unequal, it must be equal to zero or nothing. That is, the ultimate half of something is exactly equal to nothing; Q. E. D.

In his first attack on the reasoning of Sir Isaac Newton, contained in "The Analyst," Bishop Berkeley did not notice the above demonstration of the first lemma of the first book of the *Principia*. Jurin, his antagonist, complained of this neglect, and Berkeley replied: "As for the above-mentioned lemma, which you refer to, and which you wish I had consulted sooner, both for my own sake and for yours, I tell you I had long since consulted and considered it. But I very much doubt whether you have sufficiently considered that lemma, its demonstration, and its consequences." He then proceeds to point out one of these consequences, which seems absolutely fatal to Sir Isaac Newton's view of his own method. "For a fluxionist," says he, "writing about momentums, *to argue that quantities must be equal because they have no assignable difference*, seems the most injudicious step that could be taken; it is directly demolishing the very doctrine you would defend. For it will thence follow that all homogeneous momentums are equal, and con-

sequently the velocities, mutations, or fluxions, proportional thereto, are likewise equal. There is, therefore, only one proportion of equality throughout, which at once overthrows the whole system you undertake to defend."* This objection appears absolutely unanswerable. For if all quantities, which "during any finite time constantly approach each other, and before the end of that time approach nearer than any given difference, are ultimately equal," then are all indefinitely small quantities ultimately equal, since they all approach each other in value according to the hypothesis. That is to say, as zero is the common limit toward which they all continually converge, so they continually converge toward each other, and may be made to "approach nearer the one to the other than by any given difference." If, then, it follows from this that they are all "ultimately equal," "there is only one proportion of equality throughout," and the whole fabric of the infinitesimal analysis tumbles to the ground. For this fabric results from the fact that, instead of one uniform proportion, there is an infinite variety of ratios among indefinitely small quantities. If these were ultimately equal, then their ultimate ratio would always be equal to unity. But instead of always tending toward unity, the ratio of two indefinitely small quantities may, as every mathematician knows, tend toward any value between the extreme limits zero and infinity.

The objections of Berkeley, not to the method of Newton, but to Newton's view or exposition of his method, have never been satisfactorily answered. "The *Analyst* was answered by Jurin," says Playfair,

* A Defence of Free Thinking in Mathematics, XXXII.

"under the signature of *Philalethes Cantabrigiensis*, and to this Berkeley replied in a tract entitled *A Defence of Free Thinking in Mathematics*. Replies were again made to this, so that the argument assumed the form of a regular controversy; in which, though the defenders of the calculus had the advantage, it must be acknowledged that they did not always argue the matter quite fairly, nor exactly meet the reasoning of their adversary."* This is the judgment of the mathematician, not of the historian or the philosopher. No one, it seems to me, ever argued any question of science more intemperately or more unfairly than Jurin did in his reply to Berkeley. But it is not my design to enter, at present, into the merits of this controversy. I merely wish to quote Berkeley's experience among men, which so nearly coincides with my own among books. "Believe me, sir," said he to *Philalethes*, "I had long and maturely considered the principles of the modern analysis before I ventured to publish my thoughts thereupon in the Analyst. And, since the publication thereof, I have myself freely conversed with mathematicians of all ranks, and some of the ablest professors, as well as made it my business to be informed of the opinions of others, being very desirous to hear what could be said towards clearing my difficulties or answering my objections. But though you are not afraid or ashamed to represent the analysts as very clear and uniform in their conception of these matters, yet I do solemnly affirm (and several of themselves know it to be true) that I found no harmony or agreement among them, but the reverse thereof, the greatest dissonance and even contrariety of opinions,

* Progress of Mathematical and Physical Science, Part II., Sec. 1.

employed to explain what after all seemed inexplicable. Some fly to proportions between nothings. Some reject quantities because infinitesimal. Others allow only finite quantities, and reject them because inconsiderable. Others place the method of fluxions on a footing with that of *exhaustion*, and admit nothing new therein. Some maintain the clear conception of fluxions. Others hold they can demonstrate about things incomprehensible. Some would prove the algorithm of fluxions by *reductio ad absurdum*, others *à priori*. Some hold the evanescent increments to be real quantities, some to be nothings, some to be limits. As many men, as many minds; each differing from one another, and all from Sir Isaac Newton. Some plead inaccurate expressions in the great author, whereby they would draw him to speak their sense; not considering that if he meant as they do, he could not want words to express his meaning. Others are magisterial and positive, say they are satisfied, and that is all; not considering that we, who deny Sir Isaac Newton's authority, shall not submit to that of his disciples. Some insist that the conclusions are true, and therefore the principles, not considering what hath been largely said in the Analyst on that head. Lastly, several (and these none of the meanest) frankly owned the objections to be unanswerable. All which I mention by way of antidote to your false colors, and that the unprejudiced inquirer after truth may see it is not without foundation that I call on the celebrated mathematicians of the present age to clear up these obscure analytics, and concur in giving to the public some consistent and intelligible account of their great master, which, if they do not, I

believe the world will take it for granted that they cannot." *

More than one champion entered the lists against Berkeley. Besides *Philalethes Cantabrigiensis*, or Jurin, another eminent mathematician, Mr. Robins, published replies to both of the papers of the celebrated Bishop of Cloyne. But, unfortunately, in attempting to re-demonstrate the demonstrations of Newton, and clear away every obscurity from his method, the two disciples, instead of demolishing Berkeley, got into an animated controversy about the meaning of the great master. Newton, as understood by Jurin, was utterly unintelligible or false in the estimation of Robins, and, as interpreted by Robins, he was vehemently repudiated by Jurin. Now this disagreement respecting the true interpretation of Newton's interpretation of his own method is well stated by Mr. Robins.

"It was urged," says he, "that the quantities or ratios, asserted in this method to be ultimately equal, were frequently such as could never absolutely coincide. As, for instance, the parallelograms inscribed within the curve, in the second *lemma* of the first book of *Sir Isaac Newton's Principia*, cannot by any division be made equal to the curvilinear space they are inscribed in, whereas in that *lemma* it is asserted that they are ultimately equal to that space."

"Here," says he, "two different methods of explanation have been given. The first, supposing that by ultimate equality a real assignable coincidence is intended, asserts that these parallelograms and the curvilinear space do become actually, perfectly, and absolutely equal to each other." This was the view of

* A Defence of Free Thinking in Mathematics, XLIII. and XLIV.

Jurin, and it seems difficult to understand how any man could arrive at any other conclusion. Newton himself, as we have seen, expressly asserts that the "parallelograms will *in all parts* coincide with the curvilinear figure." But Mr. Robins, in his explanation, understands Newton to mean that they will *not* coincide. Newton asserts, apparently as plainly as language could enable him to do so, "the coincidence of the variable quantity and its limits," and yet the disciple denies, in the name of the master, the reality of any such coincidence. Newton declares that the variable becomes "ultimately equal" to its limit, and yet Mr. Robins insists that he must have seen they would always remain unequal. Now is this to interpret, or simply to contradict, Sir Isaac Newton's explanation of his own method? No one could possibly entertain a doubt respecting the meaning of Mr. Robins. If Newton had meant unequal, could he not have said so just as well as Mr. Robins, instead of saying equal? Or, if he did not believe in "the coincidence of the variable and its limit," could he not have denied that coincidence just as clearly as he has asserted it? It is certain that from Jurin to Whewell, and from Whewell to the present mathematicians of Cambridge, Newton has generally been understood to contend for an ultimate equality between the variable quantity and its limit. Thus, in expounding the doctrine of Newton, which he adopts as his own, Dr. Whewell says: "A magnitude is said to be *ultimately equal* to its limit, and the two are said to be *ultimately in a ratio of equality.* A line or figure *ultimately coincides* with the line or figure which is its limit." * The

* Doctrine of Limits, Book II., Definitions and Axioms.

same view, as we have already seen, is also taken by Mr. Todhunter in his Differential Calculus. It is, in fact, the doctrine and the teaching of Cambridge to the present hour, in spite of all the obscurities, difficulties, doubts, and objections by which it has never ceased to be surrounded, to say nothing of the demonstrations by which it may be refuted.

The views of Mr. Robins respecting the method of limits appear perfectly just, as far as they go; yet nothing, it seems to me, could be more ineffectual than his attempt to deduce these views from the *Principia.* The author of that treatise, says he, "has given such an interpretation of this method as did no ways require any such coincidence [between the ultimate form of the variable and its limit]. In his explication of this doctrine of prime and ultimate ratios he defines the ultimate magnitude of any varying quantity to be the limit of that varying quantity which it can approach within any degree of nearness, and yet can never pass. And in like manner the ultimate ratio of any varying ratio is the limit of that varying ratio."* Now this fails to make out his case. For the "ultimate magnitude of any varying quantity" is one of the magnitudes of that quantity, and if that magnitude is its limit, then the varying quantity reaches its limit. Nor is this all. Mr. Robins has suppressed an important clause in the definition of Newton. Newton says: "These ultimate ratios with which quantities vanish are not truly the ratios of ultimate quantities, but limits towards which the ratios of quantities decreasing without limit do always converge, and to which they approach nearer than by any

* Review of Objections to the Doctrine of Ultimate Proportions.

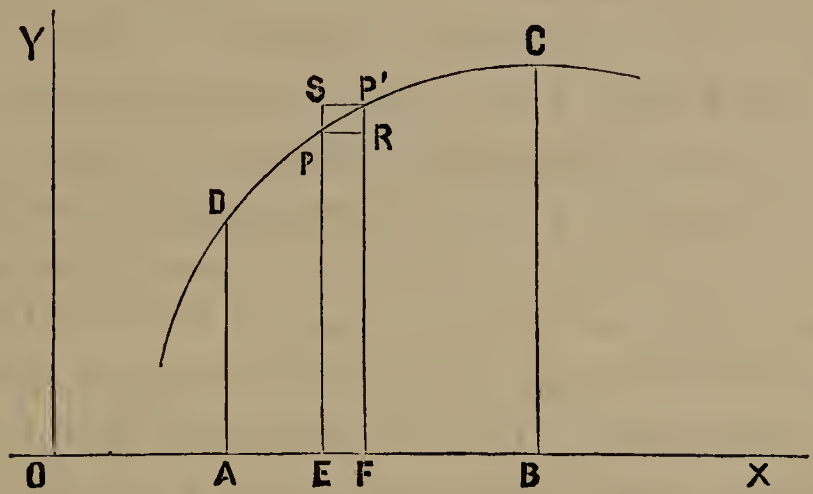
given difference, but never go beyond, *nor in effect attain to, till the quantities are diminished in infinitum.*"* Now here, in the definition of Newton as given by himself, it is said, that the varying quantity in its ultimate form attains to its limit. It was reserved for a later age to establish the truth, that a varying quantity is never equal to, or coincides with, its limit; a truth which, as we shall presently see, dispels all the obscurities of Newton's method, and places that method on a clear, logical, and immutable basis.

It is, indeed, exceedingly difficult to believe that Newton intended, by his demonstration, to establish an ultimate equality or coincidence between the parallelograms and curvilinear spaces of Lemmas II. and III.; because such an equality or coincidence seems so utterly impossible. This was the great difficulty with Mr. Robins; rather than believe such a thing of Newton, he would explain away the obvious sense of his most explicit statements. But even at the present day, after two centuries of progress in the development of the calculus and in the perfecting of its principles, the demonstration of the same paradox is frequently attempted by mathematicians of the highest rank. This demonstration is worthy of examination, not only on its own account, but also on account of the light which it throws on the operations of Newton's mind, as well as on several passages in the *Principia*. The demonstration to which I refer is usually found in the attempt to obtain a general expression or formula for the differential of a plane area. It is thus given in a very able and learned work on the Differential and Integral Calculus:

* Principia, Book II., Section I., Scholium.

"Prop. To obtain a general formula for the value of the plane A B C D, included between the curve D C, the axis O X, and the two parallel ordinates A D and B C, the curve being referred to rectanglar co-ordinates.

"Put O E $= x$, E P $= y$, E F $= h$, F P′ $= y'$, and the area A E P D $=$ A.



"Then when x receives an increment h, the area takes the corresponding increment E P P′ F, intermediate in value between the rectangle F P and the rectangle F S. But

$$\frac{\square\,\mathrm{F\,S}}{\square\,\mathrm{F\,P}} = \frac{y' \times h}{y \times h} = \frac{y'}{y} = \frac{y + \frac{dy}{dx}\cdot\frac{h}{1} + \frac{d^2 y}{dx^2}\cdot\frac{h^2}{1\,.\,2} +, \text{etc.,}}{y}$$

$$= 1 + \frac{dy}{dx}\cdot\frac{h}{y} + = \frac{d^2 y}{dx^2}\frac{h^2}{y} +, \text{ etc.,}$$

$$= 1, \text{ when } h = 0.$$

Hence at the limit, when h is indefinitely small, the area E P P′ F, which is always intermediate in value between F P and F S, must become equal to each of these rectangles or equal to $y \times h$.

$\therefore d A = y\,dx$, and consequently $A = \int y\,dx$, the required formula."*

That is, the variable rectangles F P, F S, and the intermediate curvilinear space F E P P′, are ultimately equal, or the ultimate ratio of any one of them to the other is equal to unity. As the same thing is true of all similar parallelograms and the intermediate curvilinear space, so the sum of these parallelograms or rectangles is equal to the curvilinear space A B C D, whose value is sought. Hence $A = \int y\,dx$, the sum of all the inscribed rectangles, such as E F R P. Thus it is demonstrated, as the author imagines, that the curvilinear space A B C D is equal to, or made up of, all the indefinitely small inscribed or circumscribed rectangles, such as F P. But let us look at this demonstration, which is to be found in so many works on the calculus, and examine the mysteries of its mechanism.

It is evident, in the first place, from the analytical reasoning of the author, as well as from an inspection of the figure, that the two rectangles never become equal until h, or their common base, is reduced to 0. That is to say, they never become equal as long as they are rectangles; but continue unequal until they vanish as rectangles, and become identical with their common limit, the right line E P. The curvilinear space F E P P′ becomes, at the same time, equal to and, of course, identical with the same line E P. For as long as the rectangles exist *as such*, or as long as h has any value greater than zero, the ratio of F S divided by F P is not = 1, but to

* Courtenay's Calculus, p. 330.

$$1 + \frac{dy}{dx} \cdot \frac{h}{1} + \frac{d^2 y}{dx^2} \cdot \frac{h^2}{1\,2\,y} +, \text{ etc.},$$

and this ratio becomes $= 1$ only when all three areas vanish, or become identical with the right line F P, in consequence of making $h = 0$. Hence, instead of proving that the rectangle F S is ever equal to the rectangle F P, so that $\frac{\square \text{F S}}{\square \text{F P}} = 1$, the author has only proved that the right line F P is equal to itself, F P, so that $\frac{\text{F P}}{\text{F P}} = 1$; a proposition which surely needed no proof.

But see how adroitly the reasoning is managed. "Hence at the limit," says the author, "when h is indefinitely small, the area E P P′ F, which is always intermediate in value between F P and F S, must become equal to each of these rectangles." Not at all. It is only when $h = 0$, as we have just seen in the preceding line, that the three areas vanish and become equal to the right line F P. Thus h is made $= 0$, in order to prove that the rectangles F S and F P are equal to each other, and to the curvilinear space F E P P′. But how will you take the sum of such rectangles? How will you take the sum of rectangles whose variable altitude is y, and whose base is 0? Or, in other words, how will you take the sum of right lines so as to make up an area? The truth is, as we have seen, that as h becomes smaller and smaller, the rectangles, such as F S and F P, become less and less in size, and greater and greater in number. Hence at the limit, when $h = 0$, the rectangles vanish into right lines, and the number of these lines becomes

$=\infty$. To take the sum of such rectangles, then, is only to take the sum of right lines, which throws us back two centuries, and buries us in the everlasting quagmire of the method of indivisibles.

But the author escapes this difficulty. He makes "$h=0$" in one line, or absolutely nothing, so that all quantities multiplied by it vanish, and, in the very next line, he makes h equal to an "indefinitely small" quantity. This very convenient ambiguity is, indeed, the logical artifice by which the difficulties of the calculus are usually dodged. In order to evade these difficulties nothing is more common, in fact, than to make $h=0$ on one side of an equation, and, at the same time, to make it an "indefinitely small quantity" on the other side of the same equation. The calculus before us, as well as some others, is really replete with sophisms proceeding from the same prolific ambiguity.

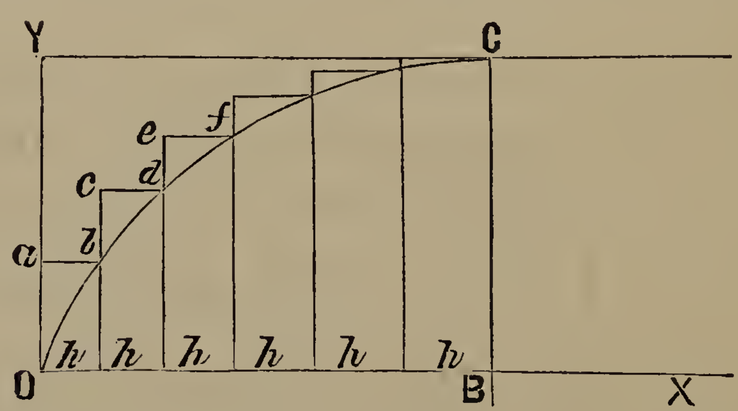
This ambiguity in the ultimate value of h, or in the method of passing to the limit of the rectangles in question, is patent and palpable in the above demonstration. It is latent and concealed in the demonstration of Newton. Neither he, nor Cavalieri, nor Robins, nor Courtenay, nor any other man, could be made to believe or imagine that the sum of any inscribed parallelogram whatever could be equal to the circumscribed curvilinear space, unless some such ambiguity, either hidden or expressed, had first obscured the clearness of his mental vision. It is evident, indeed, from the language of Newton himself, that he failed, in the demonstration of his lemmas, to effect an escape from the conception of indivisibles. It was to effect such an escape, as he tells us, that he demonstrated

the lemmas in question, "because the hypothesis of indivisibles seems somewhat harsh."* But, after all, it is clear, upon close scrutiny, that his escape from that hypothesis was far from perfect. Thus, in the fourth corollary to the third lemma, he tells us, that "these ultimate figures (as to their perimeters *a c* E) are not rectilinear, but curvilinear limits of rectilinear figures." That is to say, the ultimate form of the "evanescent parallelograms" (Cor. 1), or of the inscribed polygon (Cor. 2), or of the circumscribed polygon (Cor. 3), is not a rectilinear figure, but "the curvilinear limit" of such a figure. Now, how can the ultimate form of a polygon be "a curvilinear limit" or figure. It becomes so, says Newton, when the sides of the polygon are "diminished *in infinitum.*" But, surely, as long as its sides remain right lines it does not become a curvilinear figure. It is only when its sides have been "diminished *in infinitum,*" or ceased to be right lines, that the polygon can be conceived as coincident with a curvilinear figure. But is not this to divide the sides, or to conceive them to be divided, until they can no longer be divided? Is not this, in other words, to fall back on the conception of indivisibles —on the "somewhat harsh hypothesis" of Cavalieri? And has not the author of the *Principia,* in spite of his efforts, failed to extricate his feet at least from the entanglements of that method? Indeed, it seems utterly impossible for the human mind to escape from that method until it abandons the false principle, and the false demonstrations of the principle, that parallelograms, or polygons, or any other rectilinear figures whatever, can, by any continual division and subdi-

* Principia, Book I., Section I., Scholium.

vision, be made to coincide with a curvilinear space. The thing itself is impossible, and can only be conceived by means of "the absurd hypothesis" of indivisibles, as it is called by Carnot.

It is generally, if not universally, asserted by writers on the theory of the calculus, that the method of limits is free from the logical fault of a compensation of errors; in which respect it is supposed to possess a decided advantage over the method of Leibnitz. But this is far from being always the case. If, for example, we suppose with Sir Isaac Newton, or with Mr. Courtenay, that the inscribed rectangle, the circumscribed rectangle, and the intermediate curvilinear space are ultimately equal to each other, we can, in many cases, reach an exact conclusion only by means of a compensation of errors. In order to show this, let us resume the above general formula: $A = \int y\,dx$, which signifies that the curvilinear area A is equal to the sum of all the ultimate rectangles $y\,dx$. Now, for the sake of clearness, let us apply this formula to the parabolic area O B C, whose vertex corresponds



with the origin of co-ordinates O. Conceive the abscissa O B to be divided into any number of equal parts, and let each of these parts be denoted by h. Complete the system of circumscribed rectangles as in

the figure. Now it is evident that the sum of these rectangles is greater than the parabolic area O B C, and will continue to be greater, however their number may be increased or their size diminished, provided only that they do not cease to be rectangles. The measure of one of these rectangles in its last form is measured, as we have seen, by $y\,d\,x$, and the whole area O B C, supposed equal to the sum of these rectangles, is $\int y\,d\,x$. Now this sum, or $\int y\,d\,x$, is, I say, greater than the parabolic area in question. This may be easily shown.

From the equation of the parabola $y^2 = 2\,p\,x$, we obtain, by differentiation, $y^2 + 2\,y\,d\,y + d\,y^2 = 2\,p\,x + 2\,p\,d\,x$, or $2\,y\,d\,y + d\,y^2 = 2\,p\,d\,x$. Hence

$$d\,x = \frac{y\,d\,y}{p} + \frac{d\,y^2}{2\,p}.$$

By substituting this value of $d\,x$ in the above formula, we have the area of

$$\text{O B C, or A} = \int \left(\frac{y^2\,d\,y}{p} + \frac{y\,d\,y^2}{2\,p} \right).$$

Now this is the exact value of the sum of all the indefinitely small circumscribed rectangles. But it is greater than the parabolic area O B C; for the first term above, or $\int \frac{y^2\,d\,y}{p}$, is exactly equal to that area. For $\int \frac{y^2\,d\,y}{p} = \frac{y^3}{3p} = \frac{2\,p\,x\,y}{3\,p} = \frac{2}{3}\,x \cdot y$, the well-known value of the parabolic area O B C.

Now the sum of the parallelograms was made up of two parts, namely, of $\int \frac{y^2\,d\,y}{p}$ and of $\int \frac{y\,d\,y^2}{2\,p}$. The

first part alone, $\int \frac{y^2\,dy}{p}$, is exactly equal to the area O B C; and, consequently, the part $\int \frac{y\,dy^2}{2p}$, which was thrown away, must have been exactly equal to the sum of the little mixtilinear triangles O $a\,b$, $b\,c\,d$, $d\,e\,f$, etc., by which the sum of the rectangles exceeded the area of O B C. Hence the exact result O B C $= \frac{2}{3}\,x\,y$, was obtained by a compensation of errors; the excess of the sum of the rectangles over the area of O B C being corrected by the rejection of $\int \frac{y\,dy^2}{2p}$ as nothing. Thus, the method of Newton is not always free from a secret compensation of errors; a logical defect which has always been supposed to be exclusively confined to the method of Leibnitz.

The reason of this is, that Newton frequently mixed up the fundamental conceptions of Leibnitz with his own clearer principles, and, consequently, failed to emancipate his method from their darkening influence. This is evident from the case above considered. In the method of Leibnitz it is taken for granted that the rectangle F E P R [Fig. p. 185] may be taken for the curvilinear space F E P P′; because they differ from each other only by the infinitely small quantity P P′ R, which makes really no difference at all. This is, in fact, one of the equalities which is specified in the first postulate of the Marquis de L'Hôpital, as we saw in the last chapter of these reflections. Newton does not take this equality for granted, but he attempts to demonstrate it. But no reasoning can ever prove that

the rectangle F P, however small, is equal to the curvilinear space F E P P′; even Newton, as we have seen, failed in his attempt to demonstrate such an impossibility. Leibnitz should have said, I commit a small error in the formation of my equation by taking F P for F E P P′; but then I will correct this error by rejecting from my equation certain small quantities; for this is, in fact, precisely what he did. Newton, in like manner, should have said, I put F P in the place of F E P P′, not because they are equal, or can ever become so, but because they have the same limit; and, consequently, in passing to the limit, the same precise result will be obtained whether the one quantity or the other be used; for this is exactly what he did. But, instead of saying so, or confining their language to the real processes of their methods, both proceeded on the false conception that the infinitely small rectangle F P is exactly equal to the curvilinear space F E P P′. The only difference between them was, that Leibnitz predicated this equality of the two figures when they were infinitely small, and Newton when they had reached their ultimate form or value. Hence in the one system, as in the other, the exact result was obtained by means of an unsuspected compensation of unsuspected errors.

Again, Sir Isaac Newton wished to avoid, as much as possible, the use of infinitely small quantities in geometry. "There were some," says Maclaurin, "who disliked the making much use of infinites and infinitesimals in geometry. Of this number was Sir Isaac Newton (whose caution was as distinguishing a part of his character as his invention), especially after he saw that this liberty was growing to so great a

height."* Maclaurin himself entertained the opinion that "the supposition of an infinitely little magnitude" is "too bold a postulatum for such a science as geometry,"† and hence he commends the caution of Newton in abstaining from the use of such quantities. Indeed, Newton himself says, "Since we have no ideas of infinitely little quantities, he introduced fluxions, that he might proceed by finite quantities as much as possible."‡ But while he clung to the hypothesis, or notion, that the variable ultimately coincides with its limit, he found it impossible to avoid the use of such quantities, or else something even more obscure and unintelligible. Thus, as we have seen, he divided the sides of his inscribed and circumscribed variable polygons until he made them coincide with the limiting curve. Now, did not this make their sides infinitely small, or something less? Did it not, in fact, reduce them to indivisibles or to points? And if so, did not their length become infinitely small before it became nothing?

Nor is this all. For he says, "Perhaps it may be objected that there is no ultimate proportion of evanescent quantities, because the proportion before the qualities have vanished is not ultimate, and when they are vanished, is none. But by the same argument, it may be alleged, that a body arriving at a place, and then stopping, has no ultimate velocity, because the velocity, before the body comes to the place, is not ultimate; when it has arrived, is none.

* Introduction to Maclaurin's Fluxions, p. 2.

† Preface to Fluxions, p. iv.

‡ Philosophical Transactions, No. 342, p. 205; Robins' Mathematical Tracts, Vol. II., p. 96.

But the answer is easy, for by the ultimate velocity is meant that with which the body is moved, neither before it arrives at its last place and the motion ceases, nor after, but at the very instant it arrives; that is, that velocity with which the body arrives at its last place and with which the motion ceases. And in like manner, by the ultimate ratio of evanescent quantities is to be understood the ratio of the quantities not before they vanish, nor afterwards, but with which they vanish. In like manner the first ratio of nascent quantities is that with which they begin to be. And the first or last sum is that with which they begin or cease to be (or to be augmented or diminished). There is a limit which the velocity at the end of the motion may attain, but not exceed. This is the ultimate velocity. And there is the like limit in all quantities and proportions that begin and cease to be."* Thus, the ultimate ratio of quantities, as considered by Newton, is the ratio, not of quantities before they have vanished, nor after they have vanished, but of somethings somewhere between something and nothing. These somethings, which exist somewhere in that intermediate state, is what Bishop Berkeley has ventured to call "the ghosts of departed quantities." The ultimate ratio of two rectangles, for example, is their ratio, neither before nor after they have ceased to be rectangles, but while they are somewhere and something between rectangles and right lines. There may be, if you please, such things as such ultimate velocities or departed quantities. But, if introduced into the domain of mathematical science, will they not bring with them more of obscurity than of light?

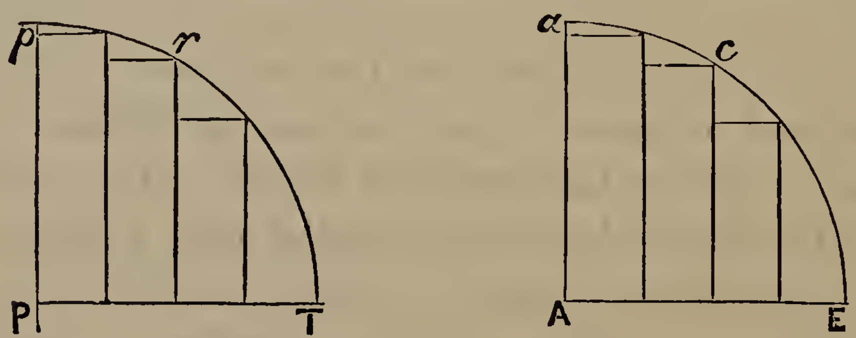
* Principia, Book I., Section I., Scholium.

"D'Alembert," says Carnot, "rejected this explication, though he completely adopted in other respects the doctrine of Newton concerning the limits or first and last ratios of quantities." * And Lagrange said, "That method has the great inconvenience of considering quantities in the state in which they cease, so to speak, to be quantities; for though we can always well conceive the ratio of two quantities, as long as they remain finite, that ratio offers to the mind no clear and precise idea, as soon as its terms become the one and the other nothing at the same time." It may be doubted, then, whether Newton gained anything in clearness and precision by the rejection of infinitely small quantities, and the invention of ultimate ones.

In order to take a complete view of Newton's method, it will be necessary to consider a few more of his lemmas, and also the object for which such dark and difficult things are demonstrated. I shall, then, begin with

LEMMA IV.

"If in two figures A *a c* E, P *p r* T, you inscribe (as before) two ranks of parallelograms, an equal num-



ber in each rank, and when their breadths are diminished *in infinitum*, the ultimate ratios of the parallelo-

† Metaphysique, etc., Chap. III., p. 182.

grams in one figure to those in the other, each to each respectively, are the same; I say, that these two figures A a c E, P p r T, are to one another in that same ratio.

"For as the parallelograms in the one are severally to the parallelograms in the other, so (by composition) is the sum of all in the one to the sum of all in the other, and so the one figure to the other; because (by Lemma III.) the former figure to the former sum, and the latter figure to the latter sum, are both in the ratio of equality. Q. E. D."

Now this demonstration, it will be perceived, proceeds on the principle that the inscribed parallelograms exactly coincide with the circumscribed curvilinear figure, and if this coincidence were not perfect then the demonstration would be defective. This proposition alone is, then, sufficient to show that Newton contended for what his words so clearly express; namely, that the inscribed parallelograms, in their ultimate form, really and rigidly coincide with the circumscribed figure. This may be very difficult to believe, but it is, nevertheless, absolutely demanded by his demonstration of the fourth lemma, as well as by his express words. Perhaps such a thing could not have been believed by any one previously to the introduction of indivisibles, and the darkness which the overstrained notions of that method introduced into the minds of the mathematical world. It is certain that if Euclid or Archimedes could have believed in such a coincidence between rectilinear and curvilinear figures, they would have had no occasion to abandon the principle of supposition, and invent or adopt the method of *exhaustion* in order to ascertain the measure of curvilinear areas.

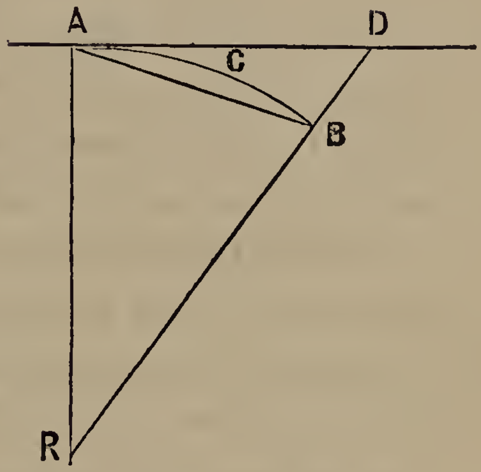
I have, it may be remembered, demonstrated in a perfectly clear and unexceptional manner a proposition similar to the above lemma, without supposing the variable to reach or coincide with its limit. That is to say, I have shown that if two variables always have the same ratio to each other, then, although they never reach their limits, yet will these limits be in the same ratio. This proposition, which entirely eschews and shuns the strained notion that a variable ultimately coincides with its limit, will be found to answer all the purposes of the fourth lemma of Newton. Even if that strained notion were true, and could be demonstrated, it would add nothing but a very unnecessary obscurity to the demonstrations of the method of limits. But Newton, as we have seen, has failed to demonstrate that strained notion, that first and fundamental conception of his method. In his attempt to do so he has, as we have seen, only shown a contradiction between two contradictory suppositions. That conception should, then, it seems to me, be for ever banished from the domain of mathematical science, as having perplexed, darkened, and confounded the otherwise transcendently beautiful method of limits.

LEMMA VI.*

"If any arc A C B, given in position is subtended by its chord A B, and in any point A, in the middle of the continued curvature, is touched by a right line

* The fifth lemma is in these words: "In similar figures, all sorts of homologous sides, whether curvilinear or rectilinear, are proportional, and the areas are in the duplicate ratio of the homologous sides." It is without a demonstration; a simple enunciation is all that the author deemed necessary.

A D, produced both ways; then if the points A and B approach one another and meet, I say, the angle B A D, contained between the chord and the tangent, will be diminished *in infinitum*, and will ultimately vanish.



"For if the angle does not vanish, the arc A C B will contain with the tangent A D an angle equal to a rectilinear angle, and therefore the curvature at the point A will not be continued, which is against the supposition."

Now this demonstration is merely preliminary to those which follow. The seventh lemma is in these words: "The same things being supposed, I say that the ultimate ratio of the arc, chord, and tangent, any one to any other, is the ratio of equality." Now this proposition is demonstrated in order to establish the practical conclusion, that "in all our reasoning about ultimate ratios, we may freely use any one of these lines for any other." [See Cor. III.]

LEMMA VIII.

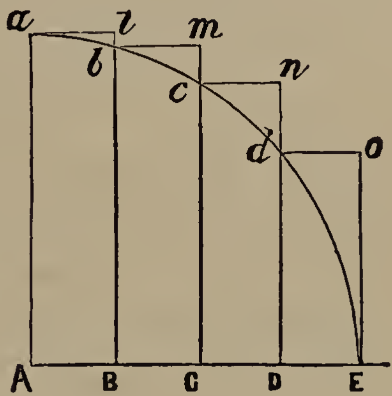
"If the right lines A R, B R, with the arc A C B, the chord A B, and the tangent A D, constitute three triangles R A B, R A C B, R A D, and the point A and B approach and meet; I say that the ultimate form of these evanescent triangles is that of similitude, and their ultimate ratio that of equality." Now this lemma is demonstrated, like the last, to establish the conclusion, that "in all our reasonings about ultimate

ratios we may indifferently use any one of these triangles for any other." [See Cor.] That is to say, it is concluded that any one of these triangles may be used for any other; because it has been demonstrated that they are "ultimately both similar and *equal* among themselves."

In this eighth lemma the "ultimate form" of these several "triangles" is in a single point. Now what, I would ask, is this "ultimate form?" Perhaps it is no form at all; perhaps it is without form and void. It is certainly contained in a point which has neither length, breadth, nor thickness. It is not the form of a triangle, for if it were, it would then be a triangle, and could not be inscribed in a point. Or, if it were the form of a triangle, it would then be a triangle that had not vanished, which is contrary to the very definition of an "ultimate triangle." Nor is it the form of a triangle after it has vanished, for then it is nothing, and has no form. What, then, is this "ultimate form" of a triangle? It is not, we are told, a triangle either before or after it has vanished, but while it is in the act of vanishing. With what form, then, does a triangle vanish? Certainly not with the form of a triangle, for then it would still be a triangle, which is contrary to the definition. Nor with the form of a point, for then it has ceased to be a triangle, which is likewise contrary to the definition. Must I conclude, then, that this "ultimate form" is some unknown form between that of a triangle and a point? It is certain that I can no more conceive of "this ultimate form of the three triangles" which are no longer triangles before they have vanished, than I can of the ultimate form of the parallelograms, which, in Lemma

II., are supposed to coincide with the curvilinear space A a E.

Now all these demonstrations are just as unnecessary as they are obscure. The sum of the inscribed and the sum of the circumscribed parallelograms in Lemma II. are never equal, and all that it is necessary to say is, not that they are equal, but that they have the same limit A a E. This is perfectly obvious, and to go beyond this is a supererogation of darkness and error. Take, for example, the system of circumscribed parallelograms, A l, B m, C n, D o, etc., and if we obtain an expression for their sum, we shall find it to consist of two terms. The one will be constant, and stand for the invariable part of the sum, namely, the area A a E; the other will be variable, and represent the variable portion of that sum, namely, the sum of all the little mixtilinear triangles $a\ b\ l$, $b\ c\ m$, etc., which is the variable excess of the parallelograms over the constant area A a E. Hence, if the variable term which represents the sum of these little triangles be rejected, the exact area A a E will be obtained, and this is precisely what is done in passing to the limit of the expression for the sum of the parallelograms. Now all this is perfectly plain and palpable. Hence, if the author had been content to say that the sum of the parallelograms is never equal to the area A a E, but that this area is the limit of that sum, then his method would have been as transparent and easy of comprehension as it is now dark and difficult to be apprehended. He saw that in the practical



application of the calculus it was necessary to use indifferently the sum of the inscribed and the sum of circumscribed rectangles for one another, or for the curvilinear space A a E; but he justified this procedure on the wrong ground. He justified it on the ground that they were all ultimately equal; whereas he should have done so on the ground that the variable sums, though never equal, have the same limit. This principle, which is so clear in the case before us, is general. For it is evident that "the limit of the sum of infinitely small positive quantities is not changed when these quantities are replaced by others whose ratios with them have respectively unity for their limit." But this general principle is, if possible, rendered still more evident by a very short and easy demonstration in Duhamel's work.*

The same thing is true in regard to the substitution of the chord, arc, and tangent for each other in the application of the calculus whenever such substitution answers the purpose of the operator. Newton justifies this substitution on the ground that these several quantities are all ultimately equal; but yet, as long as the arc has any value at all, it is greater than its chord and less than its tangent. Newton saw this, and hence, instead of stopping with Leibnitz, who pronounced these lines equal when they were infinitely small, he followed them down still further, and pronounced them equal after they had passed the bounds of the infinitely small, and ceased to have any magnitude whatever. But this view, as Lagrange said, has the great disadvantage of requiring us to consider quantities in the state in which they have ceased to be

* Vol. I., Chap. VI., p. 35.

quantities, and become—we know not what. Both Newton and Leibnitz, however, agreed to justify the using of "any one of these quantities for any other," on the ground that they became equal. The chord, the arc, and the tangent are coincident and equal when infinitely small; and hence, in seeking their ratios, they may be indifferently used the one for the other. The chord, the arc, and the tangent, said Newton, are all ultimately coincident and equal; and hence, "in all our reasoning about ultimate ratios, we may freely use any one of these lines for any other." But if we justify this substitution, or convertibility, on the true ground, every possible obscurity will vanish from the process, and Newton himself, if alive, might well exclaim, "Behold my theory, or method, resumed with more of clearness and precision than I myself could put into it!"*

This true ground is thus stated and demonstrated by Duhamel:

"SECOND THEOREM.—*The limit of the ratio of two quantities indefinitely small is not changed when we replace these quantities by others which are not equal, but of which the ratios with them have unity for their limits.*

"Let there be, in fact, two indefinitely small quantities α and β, α' and β' two other quantities such that the limits of $\frac{\alpha}{\alpha'}$ and of $\frac{\beta}{\beta'}$ may be equal to unity, and that, consequently, the limits of the inverse ratios $\frac{\alpha'}{\alpha}$, $\frac{\beta'}{\beta}$ may also be equal to unity; we shall have identically

* The exclamation of Carnot when he saw his own theory of the method of Leibnitz as propounded by Lagrange.

$$\frac{\alpha}{\beta} = \frac{\alpha'}{\beta'} \cdot \frac{\beta'}{\beta} \cdot \frac{\alpha}{\alpha'}.$$

The limits of equal quantities being equal, the limit of a product being the product of the limits,* we obtain from the above identity, in designating the limits by the abbreviation *lim.*, and observing that

$$lim.\ \frac{\beta'}{\beta} = 1 \text{ and } lim.\ \frac{\alpha'}{\alpha} = 1,$$

$$lim.\ \frac{\alpha}{\beta} = lim.\ \frac{\alpha'}{\beta'},$$

which it was necessary to demonstrate."

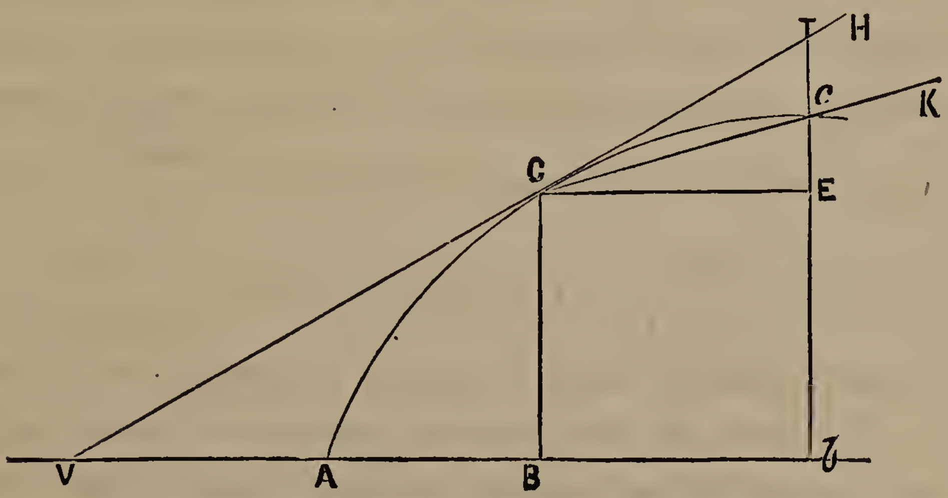
Now the chord, the arc, and the tangent when considered as small variables, or infinitesimals, exactly conform to the conditions of this important theorem. For as every one knows, the limit of $\frac{\text{chord}}{\text{arc}} = 1$, the limit of $\frac{\text{chord}}{\text{tangent}} = 1$, and the limit of $\frac{\text{arc}}{\text{tangent}} = 1$. Hence, although these lines are not equal, yet, in seeking the limit of their ratios, any one of them may be freely used for any other; because this, as just clearly demonstrated, will make no possible difference in the result.

The same thing is true of the triangles of Lemma VIII. For, as may be easily seen, the limit of the ratio of any two of these triangles = 1. Hence, in seeking the limit of their ratios, "any one of these triangles may be freely used for any other," since, according to the above theorem, this will make no

* See Demonstrations in Chap. II.

difference in the result. We thus get rid of the desperate difficulty and darkness of conceiving three triangles to be inscribed in a single point, and justify the substitution of any one of them for any other, even before they have vanished, and while they are still finite variable magnitudes, on the ground of a perfectly clear and unexceptionable principle.

I shall, in conclusion, illustrate these three several modes of viewing the infinitesimal method by an example; and I shall select the question of tangency, since it was the consideration of that question which led to the creation of the modern analysis. Let it be required, then, to determine the tangent line at the point C of the curve A C *c*. Now, as we know from



Trigonometry, the tangent of the angle C V B, which the tangent line V C T makes with the axis of *x*, is equal to $\frac{\text{B C}}{\text{B V}}$, and this, from the similar triangles C B V and T E C, is equal to $\frac{\text{T E}}{\text{C E}}$. Hence, if we find the value of $\frac{\text{T E}}{\text{C E}}$, we shall have the tangent of the

required angle B V C, and the tangent line V C T may be constructed or drawn.

The only question is, then, how to find the value of the ratio $\frac{\mathrm{T\,E}}{\mathrm{C\,E}}$. Now T E, which is the increment of B C for the tangent line, when A B is made to assume the increment B *b*, cannot be found from the equation of the tangent line, since that line has to be determined before its equation can be known. Hence, in all three methods, the line *c* E is substituted for T E, in order to find the value of the required ratio $\frac{\mathrm{T\,E}}{\mathrm{C\,E}}$. Now, the ground or principle on which this substitution is justified constitutes precisely the difference between the methods of Leibnitz, of Newton, and of Duhamel.

Let us suppose, then, that the line *b c* moves toward B C, making the lines C E, C *c*, and C T continually smaller and smaller. According to Leibnitz, when the point *c* approaches infinitely near to C, so that the arc C *c* becomes infinitely small, then the chord C *c*, the arc C *c*, and the tangent C T become coincident and equal, and consequently *c* E becomes equal to T E. Hence, he concluded that *c* E might be freely and safely substituted or taken for its equal T E. But, as we have seen, this was an error which was afterwards corrected by the opposite and equal error committed by him in throwing out certain infinitely small quantities as nothings in comparison with other quantities. Thus, although he reached the true result, finding the exact value of $\frac{\mathrm{T\,E}}{\mathrm{C\,E}}$, he did so by means of an unsuspected compensation of unsuspected errors. His

two demands, or suppositions, or postulates, or axioms were false, and yet his conclusions were correct without his ever having seen why or wherefore. Such was the method of Leibnitz.

Newton rejected the postulates of Leibnitz. He refused, as Archimedes had done before him, to consider a curve as a polygon of an infinite number of sides, or to believe in the absolute coincidence of a curve and right line, however short the two magnitudes. Hence, he denied the coincidence of the two triangles, c C E, T C E, and the mixtilinear intermediate one c C E, as long as c C retained any value whatever. Accordingly, in order to establish an identity between the three triangles in question, so as to justify the taking of c E for T E, he expressly insists, in the introduction to his *Quadraturam Curvarum*, that the point c shall not stop short of the point C, but that these two points shall become exactly coincident, or one and the same point. We are thus required to believe that a point may be considered as a triangle, or that a triangle may be inscribed in a point. Nay, that three dissimilar triangles then become "similar and equal when they have reached their ultimate form in one and the same point." Who would not be glad to be delivered from the necessity of such a belief or opinion?

Duhamel abandons the idea of any such equality. He supposes c E and T E to remain always unequal. But he still insists, nay, he demonstrates, that c E may be used instead of T E, in order to find the value of $\frac{\text{T E}}{\text{C E}}$, because *lim*. $\frac{c\text{ E}}{\text{T E}} = 1$. This is evident, for as the point c approaches the point C, it is obvious that

c E and T E become more and more nearly equal, and their difference T c approaches more and more to an equality with zero. Hence *lim.* $\frac{c\text{ E}}{\text{T E}} = 1$, and therefore in seeking the value of $\frac{\text{T E}}{\text{C E}}$, the line c E may be used for T E.

Indeed, in the case before us all this is perfectly evident without the aid of any demonstration whatever. For $\frac{\text{T E}}{\text{C E}}$, which is always constant, is evidently the limit of the variable ratio $\frac{c\text{ E}}{\text{C E}}$. For as c approaches C, the variable ratio $\frac{c\text{ E}}{\text{C E}}$ approaches in value the constant ratio $\frac{\text{T E}}{\text{C E}}$, and may be made to approach it "nearer than by any given difference." Hence, according to the definition of a limit, the limit of

$$\frac{c\text{ E}}{\text{C E}} = \frac{\text{T E}}{\text{C E}}.$$

If, therefore, we would find the value of the unknown ratio $\frac{\text{T E}}{\text{C E}}$, we only have to obtain from the equation of the curve an expression for $\frac{c\text{ E}}{\text{C E}}$, and then pass to its limit, which is the value of $\frac{\text{T E}}{\text{C E}}$, than which nothing is more easily done. Behold, then, the method of limits delivered from its obscurities, and rendered as transparent as the Elements of Euclid!

CHAPTER VIII.

OF THE SYMBOLS $\frac{0}{0}$ AND $0 \times \infty$.

IF anything in the whole science of mathematics should be free from misconception and error, one would suppose it ought to be the symbol 0, which usually stands for simply nothing. Yet, in fact, this is precisely one of those symbols which has most frequently led mathematicians from the pure line of truth, or kept them from entering upon it. "In the fraction $\frac{a}{b}$," it has been said, "if we suppose a to remain constant while b continually increases, the value of the fraction continually diminishes; when b becomes very great in comparison with a, the value of the fraction becomes very small; finally, when b becomes greater than any assignable quantity, or ∞, the value of the fraction becomes less than any assignable quantity, or 0; hence

$$\frac{a}{\infty} = 0.$$

This kind of 0 differs analytically from the absolute zero obtained by subtracting a from a, $a - a = 0$. It is in consequence of confounding the 0 arising from dividing a by ∞ with the absolute 0, that so much confusion has been created in the discussion on the

subject. About the absolute 0 there can be no discussion; all absolute 0's are equal. But the other 0's are nothing else than infinitely small quantities or infinitesimals; and there is no incompatibility in supposing that they differ from each other, and that the ratio of two such zeros may be a finite quantity."* Such is the author's interpretation of $\frac{0}{0}$. It is not zero divided by zero at all, it is only one infinitely small quantity divided by another. If so, why in the name of common sense did not the reasoner say what he meant, and, instead of calling an infinitely small quantity 0, represent it by the symbol i, or some other different from 0. Surely, it was just as easy to say $\frac{a}{\infty} = i$ as it is to say $\frac{a}{\infty} = 0$, or to write $\frac{i}{i'}$ as it is to write $\frac{0}{0}$. And then there would not have been the least shadow or appearance of the confusion of which he complains, and of which he endeavors to explore the origin.

"Logical accuracy," says the author, "would seem" to require that some other name should be given "to one of these zeros [most assuredly]; but if two meanings of the term 0 are fully understood, no trouble need arise in retaining the nomenclature which has been sanctioned by the custom of centuries." But why introduce such utterly needless ambiguities into the science of mathematics? Is it only that they may be explained in dictionaries, and carefully watched by mathematicians in order to keep out darkness and confusion from their reasonings? The truth is, there

* Dictionary of Mathematical Science, by Davies and Peck.

is no use whatever for any such ambiguity, except to explain the symbol $\frac{0}{0}$, and to dodge other difficulties of the calculus; causing it to swarm with sophisms instead of shining with solutions.*

The above explanation is easy, but it does not meet the difficulties of the symbol $\frac{0}{0}$ as it arises in the calculus. Indeed, it only deals with that symbol in the abstract, and not as seen in its necessary connections in practical operations. The author attempts this in his well-known work on the Differential Calculus. In finding the differential co-efficient of $u = a\,x^2$, he gives to x the increment h, which makes

$$u' = a\,(x + h)^2 = a\,x^2 + 2\,a\,x\,h + a\,h^2.$$

Hence $$u' - u = 2\,a\,x\,h + a\,h^2,$$

or $$\frac{u' - u}{h} = 2\,a\,x + a\,h.$$

Now, he represents "by $d\,x$ the last value of h," that is, the value of h which cannot be diminished, according to the law of change to which h or x is subjected, without becoming 0, and "by $d\,u$ the corresponding difference between u' and u." We then have, says he.

$$\frac{d\,u}{d\,x} = 2\,a\,x + a\,d\,x.$$

Now we certainly expected him to say this, but he has said, we then have

* The same learned disquisition on nothing is also found in "Davies' Bourdon," as well as in other works on Algebra.

$$\frac{d\,u}{d\,x} = 2\,a\,x.$$

What, then, has become of the term $a\,d\,x$? It appears to have vanished without either rhyme or reason. How is this mystery to be explained?

"It may be difficult," says the author, "to understand why the value which h assumes in passing to the limiting ratio is represented by $d\,x$ in the first member, and made equal to zero in the second." Truly, this is a most difficult point to understand and needs explanation. For if h be made absolutely zero or nothing on one side of the equation, why should it not also be made zero on the other side? It may, if you please, be zero or nothing sometimes, and sometimes an infinitely small quantity; but can it be both at one and the same time, and in the same operation? It is, indeed, most convenient to use h in this ambiguous sense, making it absolutely nothing on one side of an equation and very small on the other; for this gives the true result $\frac{d\,u}{d\,x} = 2a\,x$, which might not otherwise be so easily obtained; but has the author anywhere justified in his *Logic of Mathematics* a process seemingly so arbitrary? Or is the Logic of Mathematics so different from all other logic that so flagrant a solecism is agreeable to its nature? In other words, is the Logic of Mathematics so peculiar in its character that h, the same identical quantity, may be both something and nothing at one and the same time? If so, then, in spite of the author's learned treatise, there is no telling what may not happen in the Logic of Mathematics. But, for one, I shrewdly suspect that there

is no rule in arithmetic, nor in algebra, nor in geometry, nor in the calculus, by which the answer to a question may be forced without regard to the ordinary laws of human thought or sound reasoning.

"We have represented by dx," says the author, "the last value of h." That is, "the last which h can be made to assume in conformity with the law of its change or diminution without becoming zero." But why should h, in the second member and not as well in the first, obey this law of change? Why should it there, and there alone, kick out of the traces and become nothing in spite of the law of its existence? Because (the answer is easy) this is necessary to find the true result. The author, indeed, assigns another reason. "By designating this last value by dx," says he, "we preserve a trace of the letter x, and express at the same time the last change which takes place in h as it becomes equal to zero." But why should "a trace of the letter x" be preserved in the first member of the equation and not in the second? The reason is, just because dx is needed in the first member and not in the second to enable the operator to proceed with his work. The author might have fortified his position by very good authority, since Boucharlat,* as well as other writers on the Differential Calculus, have conceived the same laudable desire to preserve "a trace of the letter x" in one member of all similar equations, while they unceremoniously eject it from the other member.

But is this all that can be said by the teachers of

* The intelligent reader, even if he had not been told in the preface, would have known that Dr. Davies had freely used the work of Boucharlat.

the calculus? Must they be thus for ever foiled in their attempts to grapple with the difficulties of the very first differential co-efficient? Shall they continue thus grievously to stumble at the very first step in the path of science, along which they undertake to guide the thinking and reasoning youth of the rising generation? Shall they continue to seek and find what no other rational beings have ever found, namely, that particular value of $\dfrac{u' - u}{h}$ "which does not depend on the value of h?"* That is to say, that particular value of a fraction which does not depend on its denominator!† I think it is quite otherwise. Such misconceptions or blunders may have been unavoidable in the dim twilight of the science, or before the grand creations of a Newton or a Leibnitz had so completely emerged, as at the present day, from the partial chaos in which their great creators necessarily left them involved. But they are now anything rather than an honor to the age in which they continue to be reproduced. Some, it is to be feared, make haste to become the teachers before they have become the real students of those sublime creations.‡

* Davies' Differential Calculus, p. 17.

† The same thing is found in Mr. Courtenay's Calculus (p. 61), as well as in a multitude of others.

‡ I am sure this was the case with myself. The ignorant boy, if he has only graduated high in mathematics at West Point, is apt to presume—what, indeed, is more presumptuous than ignorance?—that he is qualified to teach the calculus; although he may never have learned its very first lessons aright, or been once taught and made to see the rational principles which lie concealed beneath its formulæ and enigmas. I had not been a teacher of the calculus long, however, before I discovered that I had almost everything to learn respecting it as a rational system of thought. Difficulties were con-

One thing appears perfectly evident to my mind, and that is, that h should be made nothing in both members of the equation, or else in neither. I must think this or else refuse to think at all. Hence, we have

$$\frac{d\,u}{d\,x} = 2\,a\,x + a\,d\,x,$$

or
$$\frac{0}{0} = 2\,a\,x.$$

But if we adopt this last form, we escape the illegitimate expression $\frac{d\,u}{d\,x} = 2\,a\,x$, with all its shuffling sophisms, only to encounter $\frac{0}{0}$, the most formidable of all the symbols or enigmas in the differential calculus. This symbol has, in fact, always been a stumbling-block in the way of the method of limits; the great and affrightful *empusa* which has kept thousands from adopting that method. Even Duhamel shrinks from a contact with it, although its adoption seems absolutely necessary to perfect the method of limits. For *if two variables are always equal, then their limits are equal.* But the limit of $2\,a\,x + a\,h$ is $2\,a\,x$, and the

tinually suggested in the course of my reflections on the subject, about which I had been taught nothing, and consequently knew nothing. I found, in short, that I had only been taught to work the calculus by certain rules without knowing the real reasons or principles of those rules; pretty much as an engineer, who knows nothing about the mechanism or principles of an engine, is shown how to work it by a few superficial and unexplained rules. This may be a very useful sort of instruction; it is certainly not mental training or education. It may be knowledge; it is not science.

limit of $\frac{u' - u}{h}$ is $\frac{0}{0}$. Hence, if we are not afraid to trust our fundamental principle or to follow our logic to its conclusion, we must not shrink from the symbol $\frac{0}{0}$. This symbol is repudiated by Carnot and Lagrange. It is adopted by Euler and D'Alembert; but they do not proceed far before it breaks down under them. It is, nevertheless, one of the strongholds and defences of the method of limits, which cannot be surrendered or abandoned without serious and irreparable loss to the cause.

Carnot thus speaks of this symbol: "The equation $\frac{\text{M Z}}{\text{R Z}} = \frac{y}{a - x}$ found in section (9) is an equation always false, though we can render the error as small as we please by diminishing more and more the quantities M Z, R Z; but in order that the error may disappear entirely, it is necessary to reduce these quantities to absolute 0's; but then the equation will reduce itself to $\frac{0}{0} = \frac{y}{a - x}$, an equation which we cannot say is exactly false, but which is insignificant, since $\frac{0}{0}$ is an indeterminate quantity. We find ourselves, then, in the necessary alternative either to commit an error, however small we may suppose it, or else fall upon a formula which conveys no meaning; and such is precisely the knot of the difficulty in the infinitesimal analysis."*

As in the problem of quadratures, the only alternative seemed to be either to commit an error with Pas-

* Reflexions, etc., Chap. I., p. 41.

cal by rejecting certain small quantities as zeros, or to find with Cavalieri the sum of an infinity of nothings, which, in the modern algorithm, is equivalent to the symbol $0 \times \infty$; so in the question of tangency the only alternative seems to lie between committing a similar error with Leibnitz, by the arbitrary rejection of infinitely small quantities in the second member of an equation as nothing, or the recognition and adoption of the symbol $\frac{0}{0}$. I have already said that, as it seems to me, there is a profound truth at the bottom of Cavalieri's conception, or in the symbol $0 \times \infty$, which has never been adequately understood or explained. Precisely the same thing appears to me perfectly true in regard to the conception of Newton, which, if properly understood, is the true interpretation of the symbol $\frac{0}{0}$.

Now the objection, which is always urged against the use of this symbol, or this form of the first differential co-efficient, is, that $\frac{0}{0}$ is an indeterminate expression, and may therefore have one value as well as another. Or, in other words, that it means all things, and therefore means nothing. This objection is repeatedly argued by Carnot, with whom the method of Leibnitz evidently ranks higher than that of Newton. "It seems," says he, "that infinitely small quantities being variables, nothing prevents us from attributing to them the value of 0 as well as any other. It is true that their ratio is $\frac{0}{0}$, which may be equally supposed a or b, as well as any other quantity whatever."*

* Reflexions, etc., Chap. III., p. 182.

Again, in reply to those who complain of a want of logical rigor in the method of Leibnitz, Carnot makes him thus retort in a feigned speech: "All the terms of their equations vanish at the same time, so that they have only zeros to calculate, or the indeterminate ratios of 0 to 0 to combine."*

Even those who, by a regard for logical consistency, have been compelled to adopt the symbol $\frac{0}{0}$ as the true expression for the first differential co-efficient, have utterly failed to emancipate themselves from the influence of the above difficulty or objection. That "symbol of indetermination," as it is always called, has still seemed, in spite of all their logic, as vague and undefined as Berkeley's "ghosts of departed quantities." Even D'Alembert himself is no exception to the truth of this remark. For, in his celebrated article on the metaphysics of the differential calculus in the *Encyclopédie*, he says: "Thus $\frac{dy}{dx}$ is the limit of the ratio of z to u, and this limit is found by making $z = 0$ in the fraction $\frac{a}{2y + z}$. But, it will be said, is it not necessary to make also $z = 0$ and consequently $u = 0$ in the fraction $\frac{z}{u} = \frac{a}{2y + z}$, and then we shall have $\frac{0}{0} = \frac{a}{2y}$?" That is to say, is it not necessary to make $z = 0$ in the first as well as in the second member of the equation? Most assuredly, in the opinion of D'Alembert, although this should bring us into actual contact with the symbol $\frac{0}{0}$.

* Reflexions, etc., Chap. I., p. 37.

"But what is it," he continues, "that this signifies?" Ay, that is the very question: what is it that this symbol signifies? Has it any sense behind or beyond that vague, unmeaning face it wears? and if it has, what is its real sense? "I reply," says D'Alembert, "that there is no absurdity in it, for $\frac{0}{0}$ can be equal to anything that we please; hence it can be $\frac{a}{2y}$." But no one ever suspected $\frac{0}{0}$ of having any absurdity in it; it was only accused of having no signification, of meaning one thing just as well as another, and, consequently, of meaning nothing to any purpose under the sun. True, if $\frac{0}{0}$ may have any value we please, then it may be equal to $\frac{a}{2y}$, if we so please; but, then, it is equally true that if we please it may be equal to any other value just as well as to $\frac{a}{2y}$. But is not this simply to repeat the objection instead of replying to it? If, we ask, what signifies $\frac{0}{0}$, Carnot replies, it signifies anything, a or b, $\frac{a}{2y}$ or $\frac{2y}{a}$, or any other quantity we may please to name, and D'Alembert repeats the reply! Is that to defend the symbol $\frac{0}{0}$ or explain what it signifies? Or, in other words, is that to remove the objection that it is a symbol of indetermination, which signifies everything, and consequently nothing?

M. D'Alembert adds: "Though the limit of the ratio of z to u is obtained when $z = 0$ and $u = 0$, this limit is not properly the ratio of $z = 0$ to $u = 0$, for that presents no clear idea; we know not what is a ratio of which the two terms are both nothing. This limit is the quantity which the ratio $\frac{z}{u}$ approaches more and more in supposing z and u both real and decreasing, and which that ratio approaches as near as we please. Nothing is more clear than this idea; we can apply it to an infinity of other cases." Now there is much truth in this second reply; but, if properly understood and illustrated, this truth will be found utterly inconsistent with the first reply of D'Alembert. If, then, we would see what the symbol $\frac{0}{0}$ really signifies, we must explode the error contained in D'Alembert's first reply (or in Carnot's objection), and bring out into a clear and full light the truth in his second reply. This will vindicate the true character of this all-important and yet much-abused symbol.

The expression $\frac{0}{0}$ is, as it stands or arises in the calculus, *not* a "symbol of indetermination." If viewed in the abstract, or without reference to the laws or circumstances to which it owes its origin, then, indeed, it has no particular meaning or signification. But nothing, as Bacon somewhere says, can be truly understood if viewed in itself alone, and not in its connection with other things. This is emphatically true in regard to the symbol $\frac{0}{0}$. If abstracted from all its connections in the calculus, and viewed in its naked

form, nothing, it is admitted, could be more indeterminate than $\frac{0}{0}$. It is, indeed, precisely this unlimited indetermination of the abstract symbol which constitutes its great scientific value. For, as Carnot himself says, "It is necessary to observe that the expression of variable quantities should not be taken in an absolute sense, because a quantity can be more or less indeterminate, more or less arbitrary; *but it is precisely upon the different degrees of indetermination of which the quantity in general is susceptible that every analysis reposes, and more particularly that branch of it which we call the infinitesimal analysis.*" * If such is, then, the true character of the symbols in every analysis, and especially in the *infinitesimal analysis*, why should it be objected against one symbol and against no other? Every one knows, for example, that x and y stand for indeterminate values as well as $\frac{0}{0}$. Why, then, should this last symbol be objected to on the ground that it is indeterminate? No one means that its value may not, in each particular case, be determined, and if any one should so mean, he might be easily refuted. The more indeterminate the symbol, says Carnot, the better, and yet it is seriously objected to the symbol $\frac{0}{0}$, that "it is a quantity absolutely arbitrary" or indeterminate! †

"I have many times," says Carnot, "heard that profound thinker [Lagrange] say, that the true secret of analysis consists in the art of seizing the various degrees of indetermination of which the quantity is

* Reflexions, etc., Chap. I., p. 18. † Ibid., Chap. III., p. 184.

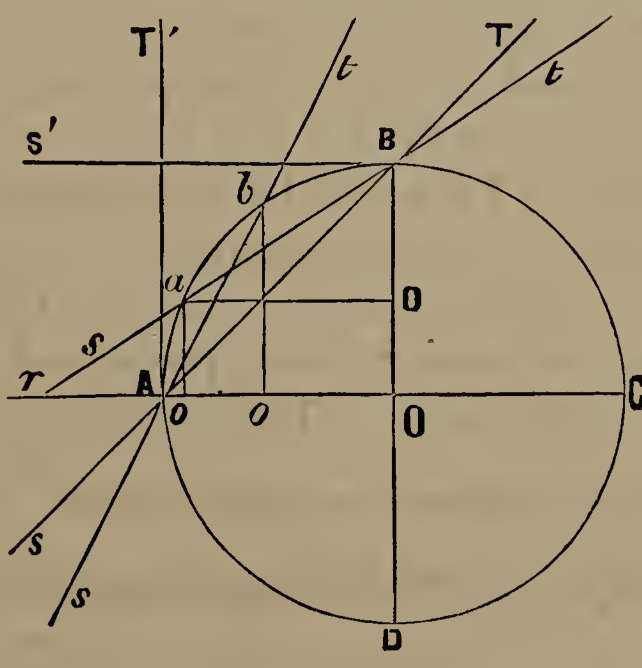
susceptible, and with which I was always penetrated, and which made me regard the method of indeterminates of Descartes as the most important of the corollaries to the method of exhaustions."* That is to say, as the most important of the methods of the infinitesimal analysis, for he regards all these methods as corollaries from the method of exhaustion. Again, in his beautiful commentary on the method of Descartes, he says: "It seems to me that Descartes, by his method of indeterminates, approached very near to the infinitesimal analysis, *or rather, it seems to me, that the infinitesimal analysis is only a happy application of the method of indeterminates.*"* He then proceeds to show that the method of Descartes, and its symbols of indeterminates, lead directly to some of the most striking and important results of the infinitesimal analysis. Surely, then, he must have forgotten the great idea with which he was always so profoundly penetrated, when he singled out and signalized the symbol $\frac{0}{0}$ as objectionable on the ground that it is indeterminate. It may, it is true, be "either a or b;" but so may x and y. These symbols may, as every one knows, be "a or b," $2a$ or $2b$, $3a$ or $3b$, and so on *ad infinitum*. Yet no one has ever objected to these symbols that they are indeterminate. On the contrary, every mathematician has regarded this indetermination as the secret of their power and utility in the higher mathematics. This singular crusade of mathematicians against one poor symbol $\frac{0}{0}$, while all other symbols of indetermination are spared, is certainly

* Reflexions, etc., Chap. III., p. 208. † Ibid., p. 150.

a very curious fact, and calls for an explanation. It shall in due time be fully explained.

So far from denying that $\frac{0}{0}$, abstractly considered, is indeterminate, I mean to show that it is, in the words of the objection to it, "absolutely arbitrary." This degree of indetermination is, indeed, the very circumstance which constitutes its value, and shows the high rank it is entitled to hold among the indeterminates of geometry. It is, in other words, its chief excellency as a mathematical symbol, that it may not only come to signify "*a* or *b*," but any other value whatever, covering the whole region of variable ratios from zero to infinity. Instead of denying this, this is the very point I intend to establish in order to vindicate the character of the symbol $\frac{0}{0}$.

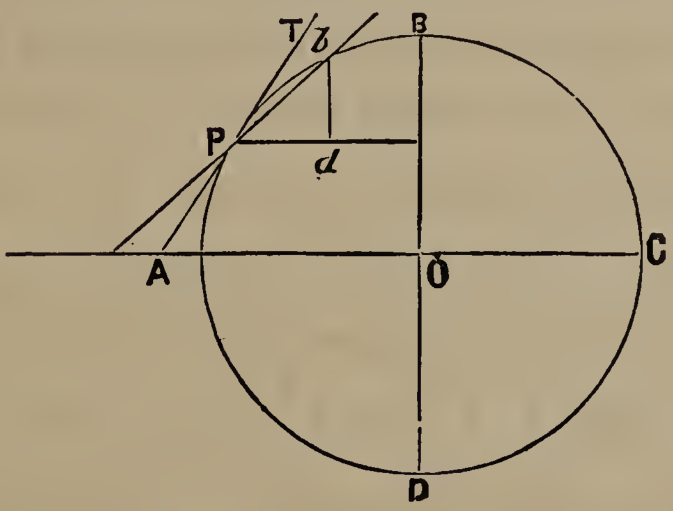
Let S T be a secant cutting the circumference of the circle in the points A and B, the extremities of two diameters at right angles to each other. Conceive this secant to revolve around the point A, so that the



point B shall continually approach A. $\frac{\text{B O}}{\text{A O}}$ is equal to the tangent of the angle B A O, which S T makes with the line A O, and in each and every successive position of the secant, such as $s\,t$, $\frac{b\ o}{\text{A}\ o}$ is equal to the tangent of the angle which it makes with A O. As B approaches A, this angle, and consequently its tangent, continually increases. That is to say, although $b\ o$ and $\text{A}\ o$ continually decrease, their ratio $\frac{b\ o}{\text{A}\ o}$ continually increases. The limit of the angle b A o is the right angle T′ A O, whose tangent is equal to infinity, toward which, therefore, the ratio $\frac{b\ o}{\text{A}\ o}$ continually tends. Hence, when the arc b A becomes indefinitely small, the angle b A o approaches indefinitely near to the right angle T′ A O, and $\frac{b\ o}{\text{A}\ o}$ approaches in value the tangent of that right angle. The secant $s\,t$ can never exactly coincide with its limit, the tangent A T′, since that tangent has only one point in common with the circumference of the circle, while the secant always has, by its very definition, two points in common with that circumference. Then, if we pass to the limit by making $\text{A}\ o = 0$, and consequently $b\ o = 0$, the equation $\frac{b\ o}{\text{A}\ o} = \tan.\ b\,\text{A}\,o$ will become $\frac{0}{0} = \tan.\ \text{T}'\,\text{A}\,o = \infty$. Again, if we conceive S T to revolve around the point B, making A continually approach toward B, we shall always have $\frac{\text{B}\ o}{a\ o} = \tan.\ \text{B}\,r\,o$. But, in this case, the

angle which the secant $s\ t$ makes with the line A 0, has zero for its limit. Hence, if we pass to the limit the equation $\frac{B\ o}{a\ o} = \tan. B\ r\ o$, will become $\frac{0}{0} = 0$.

Thus, the limit of the ratio of two indefinitely small quantities may be either infinity or zero. It is easy to see that it may also be any value between these two extreme limits, since the tangent which limits the secant may touch the circumference in any point between A and B. For example, the tangent of the



angle b P d, which the secant P b makes with P d, or A O produced, is always equal to $\frac{b\ d}{P\ d}$, as b approaches the point of contact P. Hence, if we pass to the limit, $\frac{b\ d}{P\ d} = \tan.\ b\ P\ d$ becomes $\frac{0}{0} = \tan.\ T\ P\ d$. Precisely the same relation is true in regard to every point of the arc A B. Hence, if the point of contact P be supposed to move along the arc A B from B to A, the value of the tangent of the angle T P d, or of $\frac{0}{0}$, will vary from 0 to ∞. But it should be particu-

larly observed, and constantly borne in mind, that if the question be to find the tangent line to any one point of the arc A B, then $\frac{0}{0}$ will have only one definite and fixed value, for this is an all-important fact in the true interpretation of the symbol in question.

The symbols x and y are indeterminate, just as much so as $\frac{0}{0}$. But if we suppose a particular curve, of which x and y are the co-ordinates, and make x equal to a, then y becomes determinate, and both symbols assume definite and fixed values. Now it is precisely this indetermination of the symbols x and y, abstractly considered, with the capacity to assume, under some particular supposition, determinate and fixed values, that constitutes their great scientific value. Considered as the co-ordinates of any point of any curve, x and y are of course indeterminate, absolutely indeterminate; but for a particular point of a given curve they are determinate and fixed in value. In like manner, although the symbol $\frac{0}{0}$, if considered in a general and abstract point of view, or, in other words, with reference to a tangent to any point of any curve, it is indeed absolutely indeterminate. But the moment you seek the tangent to a particular point of a given curve, the $\frac{0}{0}$ for that point has, and can have, only one value. There is, then, no more reason why this most useful symbol should be distrusted, or decried, or rejected from the infinitesimal analysis as indeterminate, than there is for the rejection of x and y or any other symbol of indetermination from the same analysis. The

very quintessence and glory of that analysis, indeed, consists in the possession and use of precisely such symbols of indetermination. Why, then, I ask again, should one be singled out and made the object of attack?

The explanation of this partial, one-sided, and slip-shod method of judgment may be easily given. In the ordinary analysis, or algebra, the symbol $\frac{0}{0}$ is not only indeterminate, but it sometimes arises under circumstances which still leave it as indeterminate as ever, failing to acquire any particular value or values whatever. This is the case in the familiar problem of the two couriers. If they start from the same point, travel in the same direction, and with the same speed, it is evident that they will always be together. Hence, if in the formula for the time when they will be together $\frac{a}{m-n} = t$, we make a, or the distance between the points of departure, $= 0$, and $m - n$, the difference between the number of miles they travel per hour, also $= 0$, we shall have, as we evidently ought to have,

$$t = \frac{0}{0}.$$

Now here the symbol $\frac{0}{0}$ remains indeterminate in the concrete, or with reference to the facts of the case, as it was in the abstract, or without reference to any particular facts or case. And the same thing is true in all cases in which a fraction, like $\frac{a}{m-n}$, becomes

$\frac{0}{0}$ *in consequence of two independent suppositions*, the one causing the numerator and the other the denominator to become $= 0$. Thus the student of mathematics becomes, in his first lessons, familiar with the symbol $\frac{0}{0}$ as not only indeterminate in the abstract, but also in the concrete. That is, he becomes habituated to pronounce it indeterminate, because it has no value in general, and can have none in the particular cases considered by him. Hence, from the mere blindness of custom (for it seems utterly impossible to assign any other reason), he continues to regard it always and everywhere in the same light. He spreads, without reflection, this view of the symbol in question over the whole calculus, and thereby blots out its real significance and utility.

In the infinitesimal analysis the symbol $\frac{0}{0}$ arises, not in consequence of *two independent suppositions*, but in consequence of *one and the same supposition*, which makes both denominator and numerator $= 0$. Thus, in the case considered by D'Alembert $\frac{z}{u} = \frac{a}{2y + z}$, z is made $= 0$, and this makes its function $u = 0$. The ratio $\frac{z}{u}$ always tends, as z becomes smaller and smaller, toward the limit $\frac{a}{2y}$, and hence in passing to the limit, by making $z = 0$, we have

$$\frac{0}{0} = \frac{a}{2y}.$$

Now in this case $\frac{0}{0}$ may not have any value as in the case of the couriers; for it has, and can have, only one value, which is $\frac{a}{2y}$. Hence D'Alembert was in error when he said that since $\frac{0}{0}$ may have any value, it may have this particular value as well as any other; for this implies that it may have any other value as well as $\frac{a}{2y}$; whereas, in the case under consideration, it must have exactly this value, and can not possibly have any other. Considered in the abstract, then, or without reference to the facts and circumstances of any particular case, the symbol $\frac{0}{0}$ may be said to be indeterminate. But yet, in very truth, this symbol never arises in the calculus without a precise signification or value stamped on its face. As it appears in the calculus, then, it is no longer indeterminate; it is perfectly clear and fixed in value. It derives this fixed value from the very law of its origin or existence, and, under the circumstances to which it owes that existence or its appearance in practice, it cannot possibly have any other value whatever.

It seems wonderful that in the very works from which $\frac{0}{0}$ is rejected as an unmeaning "symbol of indetermination," there should be methods set forth in order to find its precise value. Thus in Mr. Courtenay's Calculus, as well as in many others that repudiate the symbol in question, there is a method for find-

ing the value of $\frac{0}{0}$.* Neither he, nor any one else, ever found the value of $\frac{0}{0}$, except in reference to some particular case in which it was determinate, having assumed a concrete form. But, what seems most wonderful of all, they have a method for finding the determinate value of $\frac{0}{0}$ when that value is not obvious, and yet they assert it has no determinate value when it appears with one stamped, as it were, on its very face. Thus, if we seek the trigonometrical tangent of the angle which the tangent line to any point of the common parabola, whose equation is $y^2 = a\,x$, makes with the axis of x, we have

$$\frac{0}{0} = \frac{a}{2\,y};$$

the exact value which is made known by pure geometry. Now here $\frac{0}{0}$ arises, or appears in the calculus, with this precise, definite value $\frac{a}{2\,y}$, and yet the operator, looking this determinate value in the face, declares that $\frac{0}{0}$ has no such value. If he could not see this value, then he would apply his method to find it; but when it looks him in the face, and does not require to be found, he declares that it has no existence!

The two variable members of the equation

$$\frac{z}{u} = \frac{a}{2\,y + z}$$

* Chap. VII., p. 77.

are always equal, and hence their limits are equal. That is to say, the limit of the one $\frac{0}{0} = \frac{a}{2y}$, the limit of the other. Now here $\frac{0}{0}$ is, as D'Alembert says, not the symbol of a fraction, since zero divided by zero conveys no "clear idea." *It is the symbol of a limit.* This is its true character, and it should always be so understood and interpreted. It is the limit, the constant quantity, $\frac{a}{2y}$ (y being the ordinate to the point of contact), toward which the value of the fraction $\frac{z}{u}$ continually converges as z, and consequently u, becomes less and less.

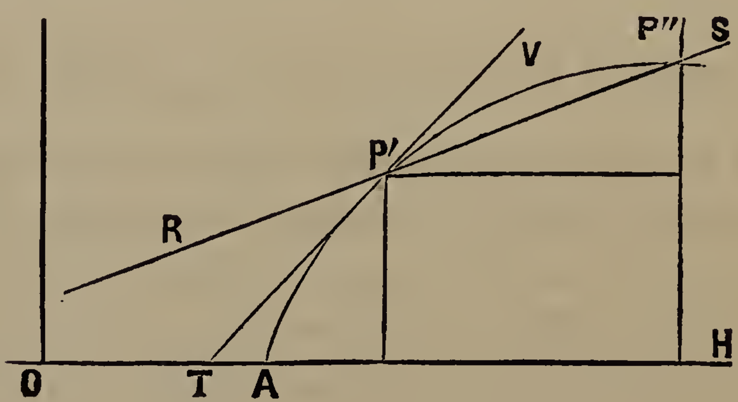
Hence there is no necessity of dodging the symbol $\frac{0}{0}$, as so many mathematicians are accustomed to do. Having reached the position $\frac{u' - u}{h} = 2ax + ah$, Dr. Davies could not say, with downright logical honesty, if we make $h = 0$, we shall have

$$\frac{0}{0} = 2ax.$$

On the contrary, he makes $h = 0$ in one member of his equation, and $= dx$, or the last value of x, in the other. By this means he preserves a trace of the letter u, as well as of the letter x, in one member of his equation, and most adroitly escapes the dreaded formula $\frac{0}{0}$. But there was no necessity whatever for any such

logical legerdemain or jugglery. For if he should ever have any occasion whatever to use this $\frac{0}{0}$, he might just substitute its value, already found, $2\,a\,x$, for it, and have no further difficulty. He might, in fact, have written his result $\frac{d\,u}{d\,x} = 2\,a\,x$, provided he had understood by $\frac{d\,u}{d\,x}$, not the last ratio of $\frac{u' - u}{h}$, but the limit of that ratio, or the constant value which that ratio continually approaches but never reaches.

It would be doing great injustice to Dr. Davies, if he were represented as standing alone in the perpetration of such logical dexterity. We ought to thank him, perhaps, for the open and palpable manner in which he performs such feats, since they are the more easily detected by every reflecting mind. It is certain that the same things are done with far greater circumspection and concealment by others, not designedly, of course, but instinctively; hiding from their own minds the difficulties they have not been able to solve. We have a notable example of this in the solution of the following problem: "To find the general differential equation of a line which is tangent to a plane curve at a given point x', y'.



"The equation of the secant line R S passing through the points $x' \, y'$ and $x'' \, y''$, is

$$y - y' = \frac{y'' - y'}{x'' - x'}(x - x') \ldots \qquad (1)$$

"But if the secant R S be caused to revolve about the point P$'$, approaching to coincide with the tangent T V, the point P$''$ will approach P$'$, and the differences $y'' - y'$ and $x'' - x'$ will also diminish, so that at the limit, where R S and T V coincide, $\frac{y'' - y'}{x'' - x'}$ will reduce to $\frac{d \, y'}{d \, x'}$, and the equation (1) will take the form

$$y - y' = \frac{d \, y'}{d \, x'}(x - x') \ldots \qquad (2)$$

which is the required equation of the tangent line at the point $x' \, y'$." *

Not exactly, for when R S coincides with T V, the point P$''$ coincides with P$'$, and the two become one and the same point. Hence, when R S coincides with T V, or the point P$''$ with the point P$'$, the equation (1) takes the form

$$y - y' = \frac{0}{0}(x - x').$$

But in order to shun the symbol $\frac{0}{0}$, which the author did not approve, he committed the error of supposing P$''$ to coincide with P$'$, without supposing the differences of their abscissas and ordinates to vanish, or become $= 0$. But most assuredly if x' is the abscissa

* Courtenay's Calculus, Part II., Chap. I., p. 148.

of P′ and x'' is the abscissa of P″, then when P″ coincides with P′, x'' will be equal to x' and the difference $x'' - x'$ will be $= 0$. The same thing happens in regard to the difference $y'' - y'$, for when the points P′ and P″ coincide, it is clear that the difference of their ordinates $y'' - y' = 0$. But the author preferred the inaccurate expression $\frac{d\,y'}{d\,x'}$ to the symbol $\frac{0}{0}$, which, in every such case, is perfectly accurate, as well as perfectly determinate. And he obtains this inaccurate expression by means of the false supposition that P′ and P″ may coincide without causing $y'' - y'$, or $x'' - x'$ to become $= 0$; which, in the application of the process to any particular curve given by its equation, is just exactly equivalent to making the increment of $x = 0$ on one side of the equation and not on the other. It is precisely the process of Dr. Davies repeated in a more covert form.

I object to the system of Dr. Courtenay, as well as to that of Dr. Davies, because they both freely use the terms *limit* and *indefinitely small* without having once defined them. Nor is this all. They habitually proceed on the false supposition that the variable reaches or coincides with its limit. Thus, in the example just noticed, it is supposed that the limit of the variable ratio $\frac{y'' - y'}{x'' - x'}$ is its last value $\frac{d\,y'}{d\,x'}$; whereas its real limit lies beyond its last value, and is accurately found only by making $y^2 - y' = 0$, and $x^2 - x' = 0$. For, as we have repeatedly seen, it is no value of the ratio $\frac{y'' - y'}{x'' - x'}$, which is equal to the tangent of the angle which the tangent line at the point $x'\ y'$ makes with

the axis of x. That tangent is equal, not to the last value of the ratio $\frac{y''-y'}{x''-x'}$, but to the limit of that ratio; a quantity which it may approach as near as we please, but can never reach. Again, they freely speak of indefinitely small quantities, and yet, in no part of their works, have they defined these most important words. But they habitually use them in a wrong sense. Instead of regarding indefinitely small quantities as variables which continually decrease, or which may be supposed to decrease as far as we please without ever being fixed or constant, they consider them as constant quantities, or as acquiring fixed and unalterable values. Thus, in the systems of both, dx, or the last value of the variable increment of x, is regarded as a constant quantity. With such conceptions, or first principles, or elements, it is impossible for the ingenuity of man to form a differential calculus free from inaccuracies and errors. All the works, in fact, which have been constructed on those principles are, like the two under consideration, replete with solecisms and obscurities. It would require much time and toil to weed them all from the calculus—at least the production of a volume.

But one more must, in this place, be noticed, both because it is very important, and because it relates to the interpretation of the symbol $\frac{0}{0}$. In the discussion of multiple points, at which of course there are several branches of the curve, and consequently one tangent for each branch, it is said, that $\frac{dy'}{dx'} = \frac{0}{0}$, since it "cannot have several values unless it assumes the indeter-

minate form $\frac{0}{0}$."* Now here, at least, the author resorts to $\frac{0}{0}$, because he cannot proceed without it, and he gives the wrong reason for its use. The truth is, if there are two branches of the curve meeting at one point, then will $\frac{0}{0}$, as found from the equation of the curve for that point, have exactly two determinate values—precisely as many as are necessary to determine and fix the positions of the two tangents, and no more. In like manner, if three or four branches of a curve meet in the same point, they give rise to a triple or quadruple point; then will $\frac{0}{0}$, obtained with reference to that point, have three or four determinate values, or exactly as many as there are tangent lines to be determined. If the secant passing through the common point first cuts one branch of the curve and then another, the $\frac{0}{0}$ found for one branch will, of course, have a different value from the $\frac{0}{0}$ obtained with reference to the other branch. Thus, such is the admirable adaptation of the symbol $\frac{0}{0}$ to all questions of tangency, that it will have just as many determinate values as it ought to have and no more, in order to effect the complete and perfect solution of the problem.

But it is a manifest error to say that $\frac{0}{0}$ is indetermi-

* Courtenay's Calculus, Part II., Chap. II., p. 191.

nate in any such case, because it has two or three or four determinate values. The truth is, we use it in such cases, not because it is indeterminate, but just because it is determinate, having precisely as many determinate values as there are tangents to be determined. These are determined and fixed in position, not by the indeterminate values of $\frac{0}{0}$, but by its determinate and determined values.

The above reason for the use of $\frac{0}{0}$ in the discussion of multiple points was assigned by Descartes, who, in the dim twilight of the nascent science, knew not what else to say; and it has since been assigned by hundreds, simply because it was assigned by Descartes. But is it not truly wonderful that it should be employed to determine two or three or more tangents at the multiple points of a curve, and yet utterly rejected as not sufficient to determine one tangent when there is one curve passing through the point? Is it not truly wonderful that it should be thus employed, because it is indeterminate, and yet rejected for precisely the same reason? It is quite too indeterminate for use, say all such reasoners, when it arises with one value on its face; but yet it may, and must be used, when it arises with two or more values on its face, just because it is indeterminate! How long ere such glaring inconsistencies and grievous blunders shall cease to disgrace the science of mathematics? Shall other centuries roll away ere they are exploded and numbered among the things that are past? Or may we not hope that a better era has dawned—an era in

which mathematicians must *think*, as well as manipulate their formulæ?

Only one other point remains to be noticed in regard to the symbol $\frac{0}{0}$. It is said, if we retain this symbol our operations may be embarrassed or spoiled by the necessity of multiplying, in certain cases, both members of an equation by 0. But the answer is easy. The first differential co-efficient, if rendered accurate, always comes out in the form of $\frac{0}{0}$; but it need not retain this form at all. Whether we use $\frac{0}{0}$ or $\frac{d\,y'}{d\,x'}$ in writing the differential equation of a tangent line to the point x', y', we shall have to eliminate $\frac{0}{0}$ in the one case, and $\frac{d\,y'}{d\,x'}$ in the other, in order to make any practical application of the formula. Now $\frac{0}{0}$ is just as easily eliminated by the substitution of its value in any particular case as is $\frac{d\,y'}{d\,x'}$, and besides its value may be found and its form eliminated by substitution without any false reasoning or logical blunder, which is more than can be said for the form $\frac{d\,y'}{d\,x'}$.

For if we write the formula in this form,

$$y - y' = \frac{d\,y'}{d\,x'}(x - x'),$$

and proceed to apply it, we shall have to commit an

error in the elimination of $\frac{d\,y'}{d\,x'}$. Suppose, for example, the question be to find, by means of this general formula, the tangent line to the point x', y' of the common parabola, whose equation is $y^2 = 2\,p\,x$. If, then, we would be perfectly accurate, we should have

$$\frac{d\,y'}{d\,x'} = \frac{p}{y'} - \frac{d\,y'^2}{2\,y'\,d\,x'}.$$

How shall we, in this case, get rid of the last term $\frac{d\,y'^2}{2\,y'\,d\,x'}$? Shall we make it zero by making $d\,y' = 0$, and yet not consider $\frac{d\,y'}{d\,x'} = \frac{0}{0}$, or shall we throw it out as if it were absolutely nothing, because it is an infinitely small quantity of the second order? Both processes are sophistical, and yet the one or the other must be used, or some other equivalent device, if we would arrive at the exact result $\frac{d\,y'}{d\,x'} = \frac{p}{y'}$; the result which is found, or rather forced, in the calculus of Dr. Courtenay,* as well as in others which have been constructed on the same principles.

Now, in the second place, suppose the general formula is written in this form:

$$y - y' = \frac{0}{0}\,(x - x').$$

We here see, by means of x', y', the point with reference to which the value of $\frac{0}{0}$ is to be found. We obtain, as in the last, the expression:

* See Part II., Chap. I., p. 150.

$$\frac{dy}{dx} = \frac{p}{y} - \frac{dy^2}{2y \cdot dx},$$

in which dy and dx are regarded as the increments of y and x, which increments are always variables and never constants. As dx, and consequently dy, becomes smaller and smaller, it is evident that the last term $\frac{dy^2}{2y \cdot dx}$ becomes less and less, since dy^2, the square of an indefinitely small quantity, decreases much more rapidly than its first power. Hence, the term in question tends continually toward its limit zero, and if *we* pass to that limit by making dx, and consequently dy, $= 0$, we shall have

$$\frac{0}{0} = \frac{p}{y};$$

or for the point x', y', we shall have

$$\frac{0}{0} = \frac{p}{y'};$$

which substituted for $\frac{0}{0}$ in the general formula, gives

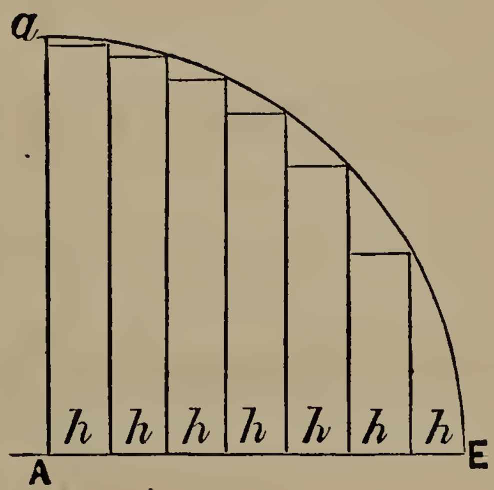
$$y - y' = \frac{p}{y'}(x - x').$$

Thus, precisely the same result is arrived at as in the former case, and that, too, without the least appearance of a logical blunder, or shadow of obscurity.

The foregoing reflections may be easily extended to the formula $0 \times \infty$, which is also called a symbol of indetermination. It is, indeed, in many cases—nay, in all cases that arise in practice—the symbol of a limit,

whose exact value may be found. There is, in Dr. Courtenay's Calculus, as well as in others, a method for finding the value of $0 \times \infty$ when this symbol does not arise with its value on its face, or on the opposite side of an equation. In every case in which its value is thus found $0 \times \infty$ is the limit toward which a variable quantity continually converges, but never exactly reaches, as any one may see by referring to the cases in the calculus of Dr. Courtenay, or of any other author.

Let us take, for example, the case considered by Cavalieri, whose conception may be expressed by the symbol $0 \times \infty$. He considered, as the reader will remember, the question of the quadrature of any plane curvilinear area. If we conceive the base A E of any such area A a E, to be divided off into equal parts, and represent each part by h, and the whole number of parts by n, and if we conceive a system of inscribed parallelograms, or rectangles, erected on those equal parts as seen in the figure, and let y represent their varying altitudes, we shall have for the sum of the rectangles the expression



$$y\, h \times n.$$

But this sum, as we have seen, is never equal to the curvilinear area A a E, though by continually diminishing the size of each rectangle, and consequently increasing the number of all, the sum may be made

to approach as near as we please to the area A a E, or to differ from it by less than any given area or space. Hence A a E is, according to the definition of a limit, the limit of the sum of the rectangles in question. As h becomes less and less, or converges toward its limit 0, n becomes greater and greater, or tends toward its limit ∞; and if we pass to the limit by making h absolutely nothing, we shall have for *the limit* of the sum of said rectangles $0 \times \infty$. Now this is not to be read or understood as zero multiplied by infinity, but simply as, in this case, the limit of $y\,h \times n$. Or, in other words, it is the symbol, not of a product, but of the limit of a sum of indefinitely small quantities whose number tends toward ∞ as their respective magnitudes tend toward 0. Accordingly, if we find the limit of that sum for any particular quadrature, we shall find the value of $0 \times \infty$ for that case, or, in other words, the exact value of the area required. Such was, at bottom, the idea of Cavalieri; but that idea was so obscurely perceived by him that he confessed he did not understand it himself. It was certainly most inadequately expressed by "the sum of lines," just as if the sum of any number of lines, however great, could make up an area or surface. Cavalieri was right in refusing to say with Roberval and Pascal, "the sum of the rectangles," because that sum is never equal to the required area. But, instead of his own inadequate expression, he should have said *the limit* of that sum, or the value of $0 \times \infty$ considered as the symbol of such limit; that is to say, provided either he or the world had been ready for the exact utterance of the truth. The mathematical world is, indeed, scarcely yet prepared for the perfect utterance

of the truth in question, so imperfectly has it understood or interpreted the symbol $0 \times \infty$, as well as the symbol $\frac{0}{0}$. To interpret these two symbols truly is, in fact, to untie all the principal knots of the Differential and Integral Calculus, and cause their manifold difficulties and obscurities to disappear.

The symbol $0 \times \infty$ may be easily reduced to the form $\frac{0}{0}$, a transformation which is effected in every complete treatise on the calculus. Thus, in Dr. Courtenay's work it is transformed: "to find the value of the function $u = \mathrm{P} \times \mathrm{Q} = \mathrm{F}\,x \times \rho\,x$, which takes the form $\infty \times 0$ when $x = a$. Put $\mathrm{P} = \frac{1}{p}$. Then $u = \frac{\mathrm{Q}}{p} = \frac{0}{0}$ when $x = a$, the common form."* He thus reduces $\infty \times 0$ to the form $\frac{0}{0}$, which he truly calls "the common form" for all the symbols of indetermination. He enumerates six such symbols, namely, $\frac{\infty}{\infty}$, $\infty \times 0$, $\infty - \infty$, 0^0, ∞^0, $1^{\pm\infty}$; all of which, in succession, he reduces to the one common form $\frac{0}{0}$, and deals with them in this form. Now, not one of these symbols has any signification whatever except as the limit of some variable expression or quantity, and since they are reducible to the form $\frac{0}{0}$, and are discussed under that form alone, it is clear that it is

* Part I., Chap. VII., p. 85.

absolutely indispensable to the correct understanding of the calculus or the doctrine of limits that we should possess the true interpretation of the symbol $\frac{0}{0}$. That interpretation is, indeed, the key to the calculus, the solution of all its mysteries. Hence the labor and pains I have been at in order to perfect that interpretation, which has not been, as some readers may have suspected, "much ado about nothing." I have always felt assured, however, that the mathematician who has the most profoundly revolved the problems of the calculus in his own mind will the most fully appreciate my most imperfect labors.

If any one has suspected that in the foregoing reflections on the philosophy of the calculus I have given undue importance to the question of tangency, from which nearly all of my illustrations have been drawn, the answer is found in the words of a celebrated mathematician and philosopher. D'Alembert, in the article already quoted, says with great truth: "That example suffices for the comprehension of others. It will be sufficient to become familiar with the above example of tangents to the parabola, and as the whole *differential* calculus may be reduced to the problem of tangents, it follows that we can always apply the preceding principles to the different problems which are resolved by that calculus, such as the discovery of *maxima* and *minima*, of points of inflexion and of "rebroussement," etc.* But, after all, the question of tangents, however general in its application, is not the only one considered in the preceding pages. The question of quadratures is likewise therein

* Encyclopédie, Art. Differential.

considered and discussed—a question which was the very first to arise in the infinitesimal analysis, and which agitated the age of Cavalieri. Yet the difficulties attending this question, which Cavalieri turned over to his successors for a solution, have, so far as I know, received but little if any attention from writers on the philosophy, or theory, or *rationale* (call it what you please) of the infinitesimal analysis. It is certainly not even touched by Carnot, or Comte, or Duhamel. Since the invention of the methods of Newton and of Leibnitz, the attention of such writers seem to have been wholly absorbed in the consideration of the theory of the problem of tangents, the one problem of the *differential* calculus, leaving the question of quadratures, which belongs to the *integral* calculus, to shift for itself, or to find the solution of its own difficulties. It is possible, indeed, to reduce the question of quadratures to a question of tangents, since, as we have seen, the symbol $0 \times \infty$ may be reduced to the form $\frac{0}{0}$; but has any one ever discussed the question of quadratures under this form, or resolved its difficulties by the use or application of any other form? Or, in other words, has any one even attempted to untie the "Gordian knot" (as it is called by Cavalieri) of the problem of quadratures? Newton, says Maclaurin, unraveled that "*Gordian knot*," and "accomplished what Cavalieri wished for."* But Newton seems to have excelled all other men in the faculty of invention, rather than in the faculty of metaphysical speculation, and hence, in his attempts to remove the difficulties of the infinitesimal analysis, he has created

* Introduction to Maclaurin's Fluxions, p. 49.

more knots than he has untied. Indeed, his own method had its *Gordian knot*, as well as that of Cavalieri, and it has been the more difficult of solution, because his followers have been kept in awe and spell-bound by the authority of his great name.

NOTES.

NOTE A, PAGE 103.

No less a geometer than M. Legendre has proceeded on the assumption that one denominate number may be multiplied by another. "If we have," says he, "the proportion A : B : : C : D, we know that the product of the extremes $A \times D$ is equal to the product of the means $B \times C$. This truth is incontestable for numbers, it is also for any magnitudes whatever, provided they are expressed or we imagine them expressed in numbers." Now, the author does not here explicitly inform us in what sort of numbers, abstract or denominate, the magnitudes should be expressed. But it is certain that they can be expressed only in concrete or denominate numbers. His meaning is elsewhere still more fully shown. For he says, "We have frequently used the expression *the product of two or more lines*, by which we mean the product of the numbers that represent the lines." . . . . "In the same manner we should understand the product of a surface by a line, of a surface by a solid, etc.; it suffices to have established once for all that these products are, or ought to be, considered as the products of numbers, *each of the kind which agrees with it. Thus the product of a surface by a solid is no other thing than the product of a number of superficial units by a number of solid units.*" Hence it appears that although M. Legendre saw the absurdity of multiplying magnitudes into each other, he perceived no difficulty in the attempt to multiply one denominate number by another—such as superficial units by solid units!

NOTE B, PAGE 130.

THE CLASSIFICATION OF LINES IN GENERAL.

Every equation, between the variables x and y, which is embraced in the general form,

$$A\, y^m + (B\, x + C)\, y^{m-1} + (D\, x^2 + E\, x + F)\, y^{m-2} +, \text{etc.}, = 0,$$

is called *algebraic*, and all others are *transcendental*. Hence lines are divided into *algebraic* and *transcendental*, according to the nature of their equations. It is only the first class or algebraic lines which are usually discussed in Analytical Geometry.

Algebraic lines are arranged in orders according to the degree of their equations. Thus a line is of the first, second, or third order when its equation is of the first, second or third degree, and so on for all higher orders and degrees. Newton, perceiving that equations of the first degree represented only right lines, called *curves of the first order* those which are given by equations of the second degree. There are certainly no simpler curves than these; but although Newton has been followed by Maclaurin, D'Alembert, and a few others, this denomination has not prevailed. By geometers, at the present day, they are universally called either lines or curves of the second order, though they are the simplest of all the classes of curves.

As we have said, the right line is the only one which an equation of the first degree can represent. No equation of the second degree can be constructed or conceived so as to represent more than three curves. These remarkable curves, thus constituting an entire order of themselves, are usually called "the conic sections" on account of their relation to the cone. No class of curves could be more worthy of our attention, since the great Architect of the Universe has been pleased to frame the system of the worlds around us, as well as countless other systems, in conformity with the mathematical theory of these most beautiful ideal forms.

But these lines, however important or beautiful, should not be permitted to exclude all others from works on Analytical Geometry. For among lines of the third and higher orders there are many worthy of our most profound attention. If it were otherwise, it would be strange indeed, since there are only three curves of the second order, while there are eighty of the third, and thousands of the fourth. This vast and fertile field should not, as usual, be wholly overlooked and neglected by writers on Analytical Geometry. The historic interest connected with some of these curves, the intrinsic beauty of others, and the practical utility of many in the construction of machinery, should not permit them to be neglected.